PROCEEDINGS

OF THE

AMERICAN SOCIETY OF CIVIL ENGINEERS

VOL. 73 FEBRUARY, 1947 No. 2

TECHNICAL PAPERS

AND

DISCUSSIONS

Published monthly, except July and August, at Prince and Lemon Streets, Lancaster, Pa., by the American Society of Civil Engineers. Editorial and General Offices at 33 West Thirty-Ninth Street, New York 18, N. Y. Reprints from this publication may be made on condition that the full title of Paper, name of Author, page reference, and date of publication by the Society are given.

Entered as Second-Class Matter, September 23, 1937, at the Post Office at Lancaster, Pa., under the Act of March 3, 1879. Acceptance for mailing at special rate of postage provided for in Section 1103, Act of October 3, 1917, authorized on July 5, 1918.

Subscription (if entered before January 1) $8.00 per annum Price $1.00 per copy

Printed in the United States of America

Current Papers and Discussions

	Published	Discussion closes
Symposium: Cavitation in Hydraulic Structures	Sept., 1945	
Discussion in Dec., 1945, Feb., Mar., Apr., May, June, Oct., 1946, Feb., 1947		Closed*
Freudenthal, Alfred M. The Safety of Structures	Oct., 1945	
Discussion in Jan., Feb., Apr., June, 1946, Feb., 1947		Closed*
Symposium: Uplift Pressure in and Beneath Dams	Dec., 1945	
Discussion in June, Sept., 1946		Closed*
Senour, Charles. New Project for Stabilizing and Deepening Lower Mississippi River	Feb., 1946	
Discussion in Sept., Nov., 1946, Feb., 1947		Closed*
Ripperger, E. A., and Davids, N. Critical Stresses in a Circular Ring	Feb., 1946	
Discussion in Oct., Nov., 1946		Closed*
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Discussion in June, Oct., Dec., 1946, Jan., 1947		Closed*
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Discussion in June, Oct., Nov., 1946, Feb., 1947		Closed*
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Discussion in Nov., 1946		Closed*
Barnett, Joseph. Express Highway Planning in Metropolitan Areas	Mar., 1946	
Discussion in May, Sept., Nov., 1946, Jan., 1947		Closed*
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Discussion in Jan., 1947		Closed*
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Discussion in Sept., Oct., 1946		Closed*
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Discussion in Oct., 1946		Closed*
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Discussion in Sept., Nov., Dec., 1946, Feb., 1947		Mar. 1, 1947
Baker, Donald M. Some Thoughts on Engineering Education	Apr., 1946	
Discussion in Sept., Oct., Dec., 1946, Feb., 1947		Mar. 1, 1947
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Discussion in Dec., 1946		Mar. 1, 1947
Dell, George H. Moment-Stiffness Relations in Continuous Frames with Prismatic Members	May, 1946	
Discussion in Oct., 1946		Mar. 1, 1947
Bermel, Karl J., and Sanks, Robert L. Model Study of Brown Canyon Debris Barrier	May, 1946	
Discussion in Nov., Dec., 1946		Mar. 1, 1947
Jacob, C. E. Effective Radius of Drawdown Test to Determine Artesian Well	May, 1946	
Discussion in Nov., Dec., 1946, Feb., 1947		Mar. 1, 1947
Report: Opportunities for Sanitary Engineers in the Postwar Period: Progress Report of the Committee on Advancement of Sanitary Engineering of the Sanitary Engineering Division	May, 1946	
Moore, R. L. Observations on the Behavior of Aluminum Alloy Test Girders	June, 1946	Apr. 1, 1947
Hansen, Howard J. Design of Plywood I-Beams	June, 1946	
Discussion in Nov., 1946, Jan., 1947		Apr. 1, 1947
Tommerup, Carl C. H. Rigid-Frame Structures Subject to Nonuniform Thermal Action	June, 1946	
Discussion in Nov., 1946, Jan., 1947		Apr. 1, 1947
Middlebrooks, T. A., and Jervis, William H. Relief Wells for Dams and Levees	June, 1946	
Discussion in Oct., Dec., 1946, Jan., Feb., 1947		Apr. 1, 1947
Report: Advances in Sewage Treatment and Present Status of the Art: Third Progress Report of the Committee of the Sanitary Engineering Division on Sewerage and Sewage Treatment	June, 1946	
Stevens, J. W. Truck Speed and Time Loss on Grades	Sept., 1946	
Discussion in Dec., 1946		Apr. 1, 1947
Underwood, P. H. Space Resection Problems in Photogrammetry	Sept., 1946	
Discussion in Dec., 1946, Jan., 1947		Apr. 1, 1947
McGuinness, Charles L. Recharge and Depletion of Ground-Water Supplies	Sept., 1946	Apr. 1, 1947
de Vries, Karl. Strength of Beams as Determined by Lateral Buckling	Sept., 1946	
Discussion in Dec., 1946, Feb., 1947		Apr. 1, 1947
Ward, C. N., and Hunt, Henry J. Correction of Tailwater Erosion at Prairie Du Sac Dam	Oct., 1946	Apr. 1, 1947
Benscoter, Stanley U. Matrix Analysis of Continuous Beams	Oct., 1946	
Discussion in Feb., 1947		Apr. 1, 1947
Machis, Alfred. Experimental Observations on Grouting Sands and Gravels	Nov., 1946	
Discussion in Feb., 1947		Apr. 1, 1947
Symposium: Cleaning and Grouting of Limestone Foundations, Tennessee Valley Authority	Dec., 1946	May 1, 1947
Matthes, Gerard A. Mississippi River Cutoffs	Jan., 1947	June 1, 1947
Tan, Ek-Khoo. Stability of Soil Slopes	Jan., 1947	June 1, 1947

Note.—The closing dates herein published are final except when names of prospective discussers are registered for special extension of time.

* Publication of closing discussion pending.

VOL. 73 PROCEEDINGS No. 2

CONTENTS FOR FEBRUARY, 1947

PAPERS

PAGE

Forecasting Productivity of Irrigable Lands.
By W. C. Muldrow 97

Reflections on Standard Specifications for Structural Design.
By Alfred M. Freudenthal 103

Development and Hydraulic Design, Saint Anthony Falls Stilling Basin.
By Fred W. Blaisdell 123

The Marine Operating Problems, Panama Canal, and the Solution.
By Miles P. DuVal 161

Sea Level Plan for Panama Canal.
By J. G. Claybourn 175

DISCUSSIONS

Cavitation in Hydraulic Structures: A Symposium.
By John K. Vennard, John C. Harrold, Jacob E. Warnock, and George H. Hickox 197

The Safety of Structures.
By Alfred M. Freudenthal 208

Torsion in Steel Spandrel Girders.
By J. E. Lothers 219

New Project for Stabilizing and Deepening Lower Mississippi River.
By Charles Senour 221

Some Thoughts on Engineering Education.
By C. A. Dykstra, and Louis Balog 224

Relief Wells for Dams and Levees.
By John S. McNown 231

CONTENTS FOR FEBRUARY, 1947 (*Continued*)

PAGE

Strength of Beams as Determined by Lateral Buckling.
By Theodore R. Higgins 236

Drawdown Test to Determine Effective Radius of Artesian Well.
By R. M. Leggette, and M. R. Lewis 239

Matrix Analysis of Continuous Beams.
By I. Oesterblom, and Harris Solman 241

Experimental Observations on Grouting Sands and Gravels.
By James B. Hays 251

A list of "Current Papers and Discussions" may be found on the page preceding the table of contents

HINTS TO AUTHORS

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AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

PAPERS

FORECASTING PRODUCTIVITY OF IRRIGABLE LANDS

By W. C. Muldrow,[1] Assoc. M. ASCE

Introduction

In 1941, Robert L. Lowry, Jr., M. ASCE, and Arthur F. Johnson, Assoc. M. ASCE, working with the United States Bureau of Reclamation, produced a heat-unit method of estimating the consumptive use of water for agriculture.[2] The writer considers this a sound and workable method, giving very consistent results from long-term records. Since the only requirement is a United States Weather Bureau record of reasonable length in, or representative of, the area under consideration, the method may be applied anywhere in the western part of the United States. The formula is easily and simply resolved, and the very consistent results, when checked against many determinations by other methods, give an accuracy satisfactory for use in preliminary estimates. As shown by J. L. Burkholder,[3] M. ASCE, in his discussion of the Lowry-Johnson paper, the line produced is the median of a band covering the range of divergence of individual years from the average.

The question of feasibility is always before the engineer on a proposed irrigation project, and feasibility is determined by the ratio between cost and returns. The three indispensable elements of a feasible project are soil, water, and climate. If an irrigable area has fertile soils, a water supply adequate to the production of full crops, and a growing season adapted to the cultivation of a pattern of profitable crops, it probably can afford a project with a relatively high per-acre price. The costs are determined by well-known engineering methods of survey and estimate. For the other side of the equation the irrigation engineer must solve the question: "What will be the returns?"

A wide variety of factors affect the crop patterns used in various irrigated communities. Accessibility to markets is an important one. Others are crop pests and diseases, the basic limitations of many crops as to their tolerance of heat and cold, etc. It is generally true that fruit and truck crops, which are most desirable in building up a diet of high nutritional value: (1) Require a

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Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by July 1, 1947.

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[2] "Consumptive Use of Water for Agriculture," by Robert L. Lowry, Jr., and Arthur F. Johnson, *Transactions*, ASCE, Vol. 107, 1942, p. 1243.

[3] *Ibid.*, p. 1287.

long season with abundant heat units to produce high yields; (2) are high-value crops, resulting in the highest per-acre incomes where they can be grown; and (3) are also high-cost crops. Since the production of these crops uses many man-hours of labor, the farm operator is not overjoyed; but this feature is desirable in a national economy where the greatest problem ahead is to provide sufficient work for all. There are also many specialty crops which are sharply limited by their very narrow environmental adaptation. Apples, citrus fruits, peppermint, and certain seed and truck crops belong to this list. Alfalfa, potatoes, the small grains, and sugar beets are among those showing a very wide range of adaptability.

With all these and many other variations in crop patterns, it is submitted that the most important attribute differentiating farm lands is the volume of crops produced. Within the same crop pattern the most valuable and profitable lands will be those able to produce the greatest tonnage per acre. From a broad water-resource planning standpoint, it is also desirable that available water be used where it will do the most good—that is, where an acre-foot of water will produce the greatest amount of crop.

A simple extension of the heat-unit method from consumptive use of water to tonnage of crop produced will provide a method of estimating the productivity of lands on a proposed project. The writer submits Fig. 1 for this purpose.

Graph

The original Lowry-Johnson consumptive use curve[4] is shown in Fig. 1 with the consumptive use points of representative localities in the irrigated west superposed. These points range from the Valley of Henry's Fork, Upper Snake River, El. 6500, down by steps to Pasco, Wash., at the junction of the Snake and Columbia rivers, then to the Sacramento Valley and on down to Imperial Valley in California, near sea level. This range, in available heat units (accumulated day degrees above 32° F), is from 4,300 to 20,000. Alfalfa is grown more universally than any other crop on these irrigated lands. In the Pacific Northwest it occupies from 35% to 60% of the acreage on nearly all projects, and is grown on substantial areas of lands in every locality shown in Fig. 1. The reason, of course, is its ability to gather nitrogen from the air in addition to its forage value. Another crop that is grown throughout this area in considerable amounts is Irish potatoes.

The acre yields used were taken from the 1940 Census,[5] where the statistics are presented by counties. The points shown in Fig. 1 were limited to localities where crops were grown in acreages large enough to give a dependable average. Two or more counties were usually added together to give coverage for the locality, and the average was taken. Some checking was done through the limited amount of crop census data available to the writer from the records of projects of the United States Bureau of Reclamation. The annual crop census taken by the Bureau on all projects forms the most accurate and detailed body

[4] "Consumptive Use of Water for Agriculture," by Robert L. Lowry, Jr., and Arthur F. Johnson, *Transactions*, ASCE, Vol. 107, 1942, p. 1252, Fig. 2.

[5] 1940 Census, Section on Agriculture, Mountain and Pacific States, Vol. 1, Pt. 6.

of crop data known to the writer. Some highly interesting and valuable conclusions can be drawn from this thorough analysis. Neither time nor comprehensive data were available to make a detailed and exhaustive study, yet it is felt that the material used is sufficient to place the yield lines shown with a fair degree of accuracy. About 2,000,000 acres of alfalfa are represented by these lines. A new irrigation project, wherever located, may be spotted on this chart by picking its total available heat units from the appropriate Weather Bureau record, and its relative productivity may be read from the yield lines.

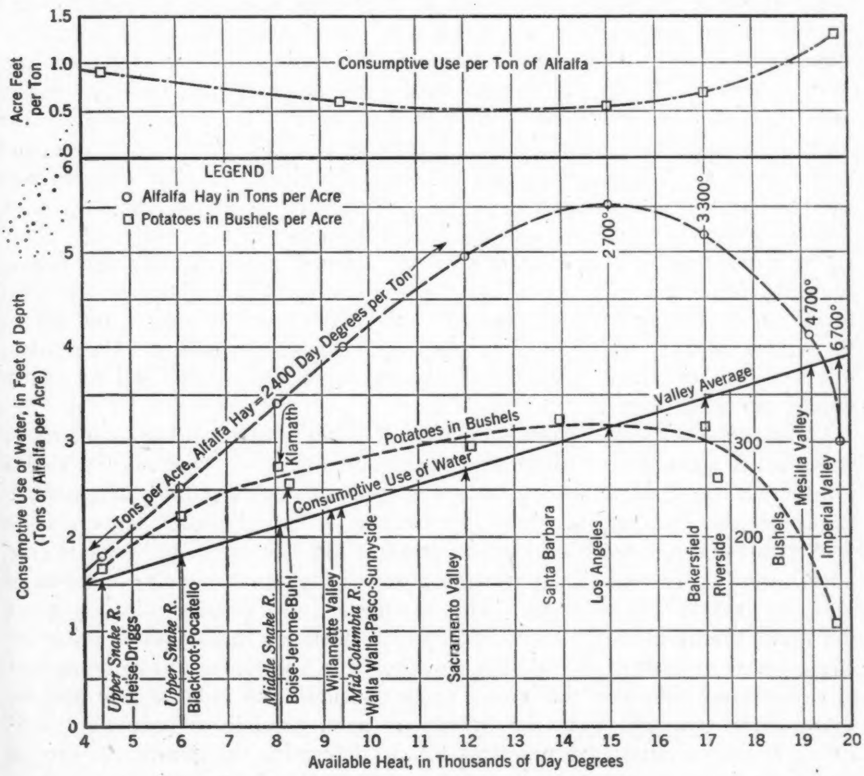

FIG. 1.—CROP YIELDS AS RELATED TO AVAILABLE HEAT UNITS AND CONSUMPTIVE USE OF WATER

Also, the relative efficiency in the use of water may be noted from the top curve, "Consumptive Use per Ton of Alfalfa." The values are naturally those for average soils producing alfalfa in each region.

It will be noted that, from Henry's Fork down to the Columbia River and Yakima Valley, the heat-unit yield relationship is a straight line, amounting to some 2,400 day degrees per ton. Practically this same value holds good in the Sacramento Valley in California; but, progressing south, into longer and hotter seasons, with more heat units, it appears that the weather becomes too hot for the crop to make effective use of all the heat available. These are questions of plant metabolism, which differs with each species. Thus the yield

decreases and the consumption of water per ton increases. The writer was quite surprised by the record in the Imperial Valley in California, where alfalfa grows every month in the year and the farmers harvest eight cuttings; yet the census shows that Imperial County had 114,146 acres of alfalfa in 1939 and that the average yield was 2.99 tons per acre. Other crop yield lines show similar characteristics. The writer traced several, but omitted them from Fig. 1 for simplicity.

It is unfortunate that some common denominator cannot be found, like total digestive nutrients, into which all the crops can be converted for any typical valley pattern; but the problem is not that simple. It is impossible directly to compare the apple orchards on the rolling hillsides of Wenatchee, Wash., with the forage and general crops of a flat and spring-frosty near-by area that may have the same total of heat units. In Yakima Valley, the Parker Heights area might produce the same tonnage of hay, potatoes, and sugar beets as do the bottom lands across the river; but the Heights also produces peaches, cherries, and pears worth several times as much per acre, because they are frost free. There are similar cases in the southwest, where citrus fruits, olives, and avocados occupy limited areas, usually the rolling foothills, while the wide plains below them are devoted to dairy and general farm crops. Imperial Valley does not grow alfalfa to make money, but rather to restore nitrogen and humus to the soils. These elements are later taken out by crops of early-season lettuce, cantaloupes, etc., which sell for many dollars per acre.

Messrs. Lowry and Johnson emphasize that their consumptive-use-of-water curve is not a simple and automatic method of solving the problem but that a full knowledge of all modifying factors is needed to develop good judgment in its use, if the best results are to be obtained. This limitation is even more true with the crop prediction method presented in this paper, although it does offer a quick way to find the relative productivity, quite closely, in terms of ordinary general crop patterns. Also, the upper curve of consumptive use per ton undoubtedly gives a relative measure of efficiency in the use of water for any given crop pattern; but a wide and thorough knowledge of agronomy and of agricultural values in the region under consideration is needed to convert that information into terms of dollars per acre, possible or probable. Irrigation engineers must have practical data to determine the question of project feasibility.

It is recognized that the crop-yield lines are only the median lines of a band, as yields are influenced in some degree by soil fertility, pests, winds, farm methods, etc. Average soil fertility has probably the greatest influence. Klamath Falls, Ore., shows a higher potato yield than Buhl, Idaho, probably because potatoes are grown on newer and richer lands. The richness of the soil solution determines the number of pounds of water a plant must drink to obtain a pound of plant food. Since plant food comes from the breakdown of extremely fine soil particles, the amount of plant food available in any soil, above the clay line, is roughly proportional to the percentage of fines in that soil. Because the alluvial soils of the Columbia River system were formed by degradation, the coarser particles stopping first and the finer particles being

carried farther downstream, it is not surprising to find this steady increase in fertility reflected in that part of this curve. In Washington the Walla Walla-Lower Yakima lands have a mean diversion duty of about 4 acre-ft; the Henry's Fork lands average 8 acre-ft; and the Lower Yakima lands consume little more than half as much water to produce a ton of alfalfa as do the Henry's Fork lands. The rise in use per ton in the hotter regions must be ascribed mainly to increased unavoidable ground evaporation which forms part of valley consumptive use. If a proposed project is restricted to high-grade lands, the yields will be greater than those read from Fig. 1, which are drawn from average lands.

Summary

This paper is submitted as a first step toward establishing a set of relationships which clearly exist and which may furnish a worthwhile tool in planning the integrated groups of irrigation and multipurpose projects that will make the best use of land and water resources along the west coast region of the United States. It is hoped that workers in this field will test it by additional data, use their knowledge and experience to correct and refine it, and so make these basic relationships as useful as possible in solving practical problems.

It is not to be expected that this method will serve as an easy substitute for detailed economic studies, in which the agricultural history of near-by and comparable developed areas is used to predict production on the project in question. The method will serve as a check on the validity of the areas selected for comparison and as an indicator of reasonable adjustments where exact parallel conditions cannot be found.

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PAPERS

REFLECTIONS ON STANDARD SPECIFICATIONS FOR STRUCTURAL DESIGN

By Alfred M. Freudenthal,[1] Assoc. M. ASCE

Synopsis

Standard specifications for structural design determine the effectiveness with which an engineer utilizes his materials; they affect the safety of structures; and they exert considerable influence upon the manufacture of materials. So powerful a tool should be handled in a manner consistent with scientific and engineering progress.

This paper attempts to rationalize the formulation of such standard specifications. It elaborates on their fundamental aspects and advocates the adoption of the scientific method in their preparation. Two examples, dealing with design live loads for highway bridges and with fatigue strength of butt welds, illustrate the arguments and procedure.

Introduction

Rational and scientific methods for designing engineering structures are comparatively new. Until the last one hundred and fifty years of recorded history, engineering was practiced as an art. Up to that time great engineering works were created by artists, whose genius of intuition was guided by sound empirical knowledge acquired through individual experience. For a long time engineering and science progressed at a different level and at a different pace. Scientists of that time, such as Euler, Hooke, Poisson, and others, whose investigations, subsequently, had a most decisive influence upon structural design and analysis, worked on an entirely different level; it took many decades before their theoretical achievements affected the engineer's methods of design. The lag between the progress of science and that of engineering—between research and practice—is a typical phenomenon of technical development; the gradual reduction of this interval indicates the increasingly effective coordination of scientific and technical progress.

Note.—Written comments are invited for immediate publication; to insure publication the last discussion must be submitted by July 1, 1947.

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Thus, engineering is being gradually transformed from a profession based on tradition and individual experience to one relying more and more on the methodically selected and classified collective experience provided by that eminently socializing agent, science. However, with this shifting of emphasis, engineering has not become an exact science—it remains a social activity. Evidently, then, it cannot dispense with subjective judgment which, while supplementing the judgment, based on objective evidence and knowledge, must not be allowed to supersede it. Only where the objective approach has not been discerned will it be necessary to rely upon subjective judgment alone.

Organization of Engineering Science

Knowledge is obtained directly by perception or indirectly by argument. Since such knowledge as is perceived or experienced directly through sensations, by observation or measurement, furnishes the premises of that part derived by argument, experience represents the sole source of knowledge. From the premises of direct experience attempts are made to justify some degree of rational belief about all sorts of inferences and conclusions by perceiving logical relations connecting them. Terms such as "certain" and "probable" describe the different degrees of this belief according to the volume of experience supporting it. The purpose or aim of these inferences and conclusions is to organize, for subsequent application, factual knowledge—using interpretation, generalization, and abstraction—mostly in the form of data or mathematical functions describing the regularities and recurrences of the observed phenomena.

Fundamentally, it makes no difference, however, whether the formulation is mathematical or verbal. There is no particular virtue in mathematical functions or in numbers as such. If they are not really representative, they can be even more misleading than verbal statements; because, psychologically, the number or formula is bound to give the impression of accuracy.

Much engineering knowledge is still descriptive, although its presentation is mathematical. As in the case of the genuinely descriptive marine or foundation engineering, there is always an undisguised, immediate, and real experience as the basis of every generalization. The development toward an exact science by using mathematical abstractions and mathematical language, however, leads occasionally to the ascendency (or domination) of the mathematical form over the real physical content—to an overvaluation of the mathematical exactness inherent in an expression of physical reality and, consequently, to a serious distortion of perspective. To avoid the pitfalls of the mathematical approach to engineering problems, it is essential to realize and to check on its limitations.

The significance attributed to information expressed by numbers or mathematical functions is an indication of the level of scientific organization of experience. At the level of descriptive science, experience is purely qualitative and, therefore, not measurable. Most knowledge has not advanced beyond this stage. A higher level is reached when methods of measuring assumedly relevant, recurrent phenomena have been developed and when the resulting figures and relations are used to devise a quantitative, although empiric, classi-

fication of experience. Numerical data have still no absolute significance; they are useful only as far as they are suitable to delimit certain classes of phenomena, and their relative significance is determined by the rigor of the classification which they express. The number or the mathematical function reaches a significance of its own only at the highest level of organization of knowledge—at that of exact science, where the relations between sets of causes and resulting effects have been so reliably established that their expression in mathematical language is justified.

Only a small part of technical science has attained this level; even in structural engineering, the methods of which are relatively exact, data are frequently utilized that do not even belong to the empiric level—as when specifying a definite number for elongation at rupture of structural steel. Although this number may be determined by a well-defined measurement belonging to the sphere of exact physical science, it does not represent more than an arbitrary conventional classification of the material with regard to a desired structural performance, based on the assumption of a vague correlation with such performance. At best it can be used only for purposes of practical designation of the material; it therefore belongs to a stage of descriptive science where measurements of phenomena already play an important part, but where rational although empiric, correlation with essential physical properties is still nonexistent. Such data should not be invested with a meaning beyond what can be justified by the physical reality behind them.

However, even where mathematical presentation is justified, the data or the function can be expected to fit the observed reality only as closely as by the margin of uncertainty inherent in the process of sampling and observation and by the margin of error detectable with the instruments used. Hence, with both the extension of the observational range and the perfection of instruments, the accuracy and reliability of such data and mathematical formulas will undergo continual improvement. Moreover, the regularity and recurrence of physical phenomena, quantities, and constants will never be perfect in a numerical sense. This fact requires the replacement of the conventional concepts of one-valued functional correlation by concepts embodying the uncertainty inherent in any observation, abstraction, or presentation of physical phenomena—where both constants and laws are considered to be of a statistical character and where the closest approach to constancy which a physical quality may attain is understood to be such that it may be represented by a frequency distribution for which all assignable causes of variation have been eliminated.

Ultimately it is of no consequence whether the fluctuations are real, objective, and inherent in the quality considered or are apparent and caused by deficiencies in the procedure of observation or measurement. Thus, for example, the strength of a material, all things being equal, should be a fairly regular and recurrent physical quality. No two samples, however, will show exactly the same test strength, except by pure chance. Fluctuations may result from differences in the quality of the material forming the individual samples and from lack of uniformity within the samples. They may, however, result from the method by which the samples have been taken and prepared; from the procedure of testing; from inaccuracies of observation and measurement; or

from a combination of these. The difficulties increase with increasing complexity of the phenomenon or the quality considered or of the sampling and testing procedure. Whereas, for instance, the results of the static tension test of steel are fairly uniform and interpretable, those of fatigue tests display irregularities and frequently become inconclusive. The question, then, is whether such lack of uniformity is an inherent characteristic of the fatigue strength of the material or is the result of the sampling and testing operation.

Generally engineering research is not so much concerned with the behavior of individual specimens of a certain group as with the behavior of the group itself. "Weight" or "strength" are abstractions not intended to represent the real measurable quality of an individual sample, but rather of the "bulk" of the material or of the statistical "universe" of all members of the group—such, for example, as the structural elements or shapes manufactured to one and the same specification in a number of plants. Similarly, "load" in a structural sense refers to the "universe" of states of loading which, for a specified group of structures, may be expected to occur. Since the behavior of a "bulk" or "universe" can be represented only by a distribution curve of statistical frequencies, any law describing the relation of real physical qualities will, to a certain extent, be statistical even if, fundamentally, its character is that of an exact law.

The Standard Specification

These basic aspects of engineering science have a particular bearing on the preparation of standard specifications for structural design. The designer relies on the specifications for all relevant information about the fundamental assumptions and premises of his design. He cannot and should not be expected to ascertain, select, and appraise, for each structure individually, the basic facts and conditions, such as load or permissible stresses, by which both the safety and the economy of the structure will be determined. This is because only in exceptional cases will he command the wide knowledge and experience required for such selection and for the appraisal of all implications. With the increasing complexity of engineering problems he will have to rely increasingly on the organized collective experience of the profession and to substitute the considered objective judgment of the group for his individual opinion.

The standard specification represents this judgment: It is the result of a collective effort of the leading groups within the profession and attempts to convey the quintessence of factual knowledge and engineering experience; it is arrived at by the application of objective methods of research and by rational interpretation of the results. Its purpose is to relieve the designer of the responsibility of selecting the premises and assumptions of the analysis of an individual structure, and to do so by establishing the fundamental relations and conditions pertaining to the design of large groups of structures having certain features in common, such as the material, the type of load, or the structural shape. This procedure has so far been successful: The number of structures designed, rather effectively, by engineers with no more than preliminary experience is considerable.

However, the rapid expansion of the engineer's knowledge and the complexity of his tasks increase the scope and difficulties of the problems to be

solved and of the decisions to be reached collectively. The soundness and correctness of these decisions, embodied in and conveyed by the respective standard specifications, is therefore becoming ever more important. Because of the extensive and differentiated knowledge required, the questions arise as to whether the methods by which collective decisions have been reached are sufficiently effective and as to whether subjective judgment and belief, unsupported by adequate objective evidence, is not allowed to play too important a part.

Standard specifications determine the efficiency of the utilization of engineering materials; they decisively affect the safety of structures; and they exert considerable influence upon the manufacture of technical materials. Thus, there is every reason to require that so powerful a tool be handled not merely with judicious care and caution, but in a manner consistent with engineering progress. This is particularly important because the utmost effectiveness in the utilization of basic materials is no longer a matter of individual judgment, but one of national importance.

Among the subjects of fundamental character covered by specifications for structural design are: (1) Design loads; (2) resistance of structural members and shapes and their permissible stresses; and (3) performance, selection, and quality control of structural materials. These are interdependent and must be coordinated. Thus, a specified permissible stress has real meaning only in conjunction with the specification of the design load and the method of structural analysis.[2] The conventional procedure by which standards for design loads, for permissible stresses, and for classifying and selecting materials are devised independently and frequently by different groups—administrative bodies specifying loads; professional societies dealing with matters of structural analysis and strength; and organizations engaged in the testing of manufactured products handling the selection of materials and control requirements—should be replaced by a process coordinating all relevant data and information. Only such a process would result in balanced standard specifications.

A rational approach to any problem requires the appraisal of the meaning of the terms and concepts, since meanings often are not what they appear at first sight. A very effective tool for this purpose is P. W. Bridgman's "operational analysis"[3]—namely, making an analysis of what is actually done when using the concept leads to a real understanding of its meaning. Using this method for the analysis of the concept of "standard specification for design," it is found that the "operation" consists in applying organized fundamental knowledge, derived from past experience and presented in a form most suitable for practical use, to the design of new structures which are expected to serve a definite purpose during a period extending into a more or less distant future. Application of past experience to the design of structures for future service requires prediction of future development (mainly of load and traffic volume), of changes of resistance, and of the state of maintenance and of the deteriora-

[2] "The Safety of Structures," by Alfred M. Freudenthal, *Proceedings*, ASCE, Vol. 71, October, 1944, p. 1157.

[3] "The Intelligent Individual and Society," by P. W. Bridgman, The Macmillan Co., New York, N. Y. 1938, pp. 74–75.

tion of the structure. These latter are matters for conjecture and represent, therefore, essentially problems for subjective judgment.

The "operational" definition shows that the first step in preparing specifications is the collection of past experience. To apply such experience effectively, the salient and recurrent phenomena should be selected and classified to derive from them the relevant relationships either by statistical inference or by induction. Each of the two approaches leads to a different kind of knowledge: Reasoning by induction, justified only if the factual evidence is conclusive, leads to knowledge of principles and to definite functional relationships of general validity; statistical inference, dealing with arguments which are not conclusive, but about which a certain degree of belief may rationally be entertained because it is supported by an appropriate amount of direct evidence, leads to knowledge of facts only and to correlations between certain variables. The reliability of these approaches is determined by the amount of evidence available; the range of validity is restricted to the original range of observation.

It is not always easy to draw a line between the two approaches because of the rôle played by the concept of chance. "Chance" may be subjective as well as objective. Subjective chance is a measure of ignorance of principles, the knowledge of which would enable one to predict the occurrence of certain events. If, however, such events are brought about by a coincidence of causes and circumstances so numerous and so complex that knowledge of principles leading to prediction is altogether out of mental reach, this is defined as objective chance. Because it is often difficult to determine whether the chance happenings entering into experience have a subjective or an objective character, the boundary between logical induction and statistical inference remains indistinct.

"Past Experience" and Experiment

The data that represent "past experience" are obtained either (1) by statistical evaluation of observations of phenomena which bear directly on the quality considered (such as traffic records or wind velocity records for the determination of service loads); (2) by observations of the behavior of existing structures (service records); or (3) by the interpretation of the results of tests or experiments designed to supply the desired information. The testing of full-scale models of structures is mostly impracticable because of their size and the complexity of service conditions—the cost of reproducing which is prohibitive. "Past experience" will, therefore, have to be based upon tests of suitably designed and selected comparatively small specimens. Such tests will be rationally interpretable if they have structural significance—that is, if the specimens tested can be considered as models, having been designed in accordance with the relevant laws of mechanical (not merely geometrical) similitude. Tests will result in the acquisition of relevant factual evidence only if:

(*a*) They can be rationally correlated with the problem under consideration;
(*b*) The characteristics observed are real physical properties, measurable in fundamental units, so as to make the tests easily reproducible and comparable with similar experiments;

(*c*) No oversimplification has been introduced by neglecting or eliminating variables or groups of variables, the relevance of which has not been reliably ascertained; and

(*d*) They have been repeated frequently enough to control the variabilities by statistical methods in order to recognize their physical significance.

Conventional tests are mostly comparative; their structural significance is doubtful or vague, as is their correlation with actual structural performance. With regard to structural materials, modern technological skill in securing specified properties is as yet not equaled by ability to discern, to specify rationally, and to control the properties which are desirable with regard to the contemplated performance in a structure.

Since past experience is to be applied to present construction, the expected behavior of the structure must be predicted on the strength of such experience. This requires the introduction of control: Making sure that what is being done is what is intended following available experience and knowledge. Obviously, such control cannot be performed by direct observation of the relevant characteristics, such as the actual resistance of the structure, but only by observing and recording one or a number of easily measurable qualities which, singly or jointly, can be considered representative. The selection of representative qualities and of adequate testing methods is, therefore, the first step in the establishment of control. The second is the selection of representative samples and the specification of their number and of the manner in which they are to be taken, so as to enable rational conclusions about the quality of the "bulk" or the "universe."

Correlation

The possibility of correlating the results of tests of a comparatively small number of samples with the quality of the "bulk" or "universe" is conditional on the imposition of adequate, uniform, and permanent control of the variabilities of the relevant characteristics upon each and every stage of the manufacturing process of the structure, from the production of the raw material to the final process of assembly.

When devising representative control tests, one has to rely on: (1) A strict functional relationship between the relevant characteristic and the observed property, (2) a statistical correlation, or (3) merely an intuitive qualitative correlation. In practical application, the difference between relationships (1) and (2) is one of grade rather than one of principle—a strict functional relationship usually being considered the limit of perfection of a statistical correlation. Also, the number of standard tests based upon a functional relationship between the representative and the represented qualities is surprisingly small, and most tests are based on an intuitive correlation. The general application of statistical methods in analyzing and drawing up standard specifications is one of the conditions of the rational utilization of materials.

Statistical methods have two different functions: One descriptive, concerned with the concise presentation of the variabilities of certain characteristics of observed phenomena; and the other inductive, with the aim of extending and utilizing such description and presentation in the generalization and the pre-

diction of the variabilities in the future. It is essential not to lose sight of the distinction since, frequently, having found a complete and satisfactory method of description, the engineer becomes less careful about the transitional arguments by which he attempts to justify the subsequent use of this description for purposes of generalization and prediction.

The ability to discriminate between the descriptive and the inductive use of statistics, as well as between functional, statistical, and intuitive correlation of phenomena, acquired by frequent and judicious application of statistical methods, is important. The engineer thus acquires not merely the habit of separating assignable causes of variation of a phenomenon from chance variabilities, but also the faculty of discriminating between subjective and objective chance causes, without losing sight of the arbitrary nature and of the expediency involved in all technical applications. He will also learn necessary caution when generalizing from a limited amount of experience. Particularly in the preparation of standard specifications will this intuitive familiarity with the possibilities as well as with the pitfalls of the statistical methods prove important.

As an illustration of the preceding arguments an attempt will be made to establish a rational approach to the drafting of two typical standard specifications for structural design. Since the data on which the examples could be based are not exhaustive, these examples are suggestive of the method applied rather than conclusive with regard to the results.

Example 1. Live Load For Medium and Long-Span Highway Bridges

The design live load for short-span highway bridges or bridge members is represented by the vehicle with the most severe concentration of weight—that is, by a fully loaded truck traveling at high speed. However, for medium and long spans the effect of the individual load concentration is small compared with the effect of the total weight of the traffic properly distributed over the respective influence line; a uniformly distributed design load will therefore represent actual loading conditions. This design load is affected by the character of the traffic; by its distribution, density, and speed; by the number of traffic lanes; by the expected period of service; and by probable changes in the traffic characteristics during the period of service. Information about the heaviest types of vehicles is insufficient, and the fact that the existing specifications convey only this information—apart from a more or less arbitrary impact factor—is a striking illustration of their inadequacy.

The types of vehicles using the modern highway are the passenger car, the passenger bus, the single truck, and the power unit or tractor with semitrailer or trailer. Denoting the loaded weights of the various groups of vehicles by $W_1, \cdots, W_n$, their empty weights by $W'_1, \cdots, W'_n$, the average frequencies of occurrence of the various groups by $F_{V1}, \cdots, F_{Vn}$, the average frequencies of occurrence of the loaded vehicles of a certain group by $F_{L1}, \cdots, F_{Ln}$, and the total number of vehicles which can be accommodated on all lanes of the bridge by n and their over-all length by L_n, the design live load, (w_{LO}) can be expressed by:

$$w_{LO} = \frac{1}{L_n} [F_{V1} F_{L1} W_1 + F_{V1} (1 - F_{L1}) W'_1 + \cdots \\ F_{Vn} F_{Ln} W_n + F_{Vn} (1 - F_{Ln}) W'_n] \qquad (1)$$

Every term of this expression is to be considered a frequency distribution. Eq. 1 represents the static weight of the design load, the vehicles being placed along the traffic lanes close to each other. The real traffic load is, however, not static but travels over the structure at a certain speed, exerting dynamic influences in addition to its static weight. At the same time the distances between the consecutive vehicles will increase with their traveling speed, resulting in a reduction of the specific load. If both the load-increasing effects and the load-reducing effects of the traveling speed are combined into one factor, I, for which the conventional term "dynamic increment" may be used, then dynamic design load, $w_L = w_{LO}\,(1 + I)$.

An expression may now be introduced for the load-increasing effects and load-reducing effects as functions of the traveling speed, v^2. Let the impact increment and its range of fluctuation be a function of the number of vehicles n:

$$I'(v) = \bar{I}'(v)\left(1 \pm \frac{1.0}{\sqrt{n}}\right) \qquad (2a)$$

in which

$$\bar{I}'(v) = \frac{1.50}{(1 + V_o/3v)\,\sqrt{n}} \qquad (2b)$$

Also, average spacing between consecutive vehicles (center to center),

$$l(v) = l_o(v)(1 \pm 0.20) \qquad (3a)$$

in which $V_o = 10$ miles per hr; and $L_o = 40$ ft; and

$$l_o(v) = \frac{L_o}{1 - 0.15\,v/V_o} \qquad (3b)$$

The chance fluctuations of $l(v)$ about its average $l(v)$ can be assumed to reach $\pm 60\%$, so as to include zero distance between consecutive vehicles as an extreme range.

Hence,

$$(1 + I) = [1 + I'(v)]\,\frac{L_n}{n\,l(v)} \qquad (4)$$

In this expression $I'(v)$, L_n, and $l(v)$ are frequency distributions. The quantity $\frac{1 + I'(v)}{l(v)}$, reaches its maximum for $v = 11.5$ miles per hr. This can be considered the critical traveling speed of the design load for bridges, accommodating more than two traveling trucks per lane, corresponding to spans exceeding 75 ft.

The weight distribution diagrams of the principal type of vehicle, the single truck, can be deduced from traffic surveys[4] and studies[5] conducted by the United States Public Roads Administration. These surveys also show that the proportion of trucks of all types (including passenger buses) is normally between 10% and 25% of the total number of vehicles and that the proportion

[4] *Public Roads*, Vol. 15, 1934, pp. 243–247; Vol. 16, 1935, pp. 17–31, 68–74, 225–237, and 238–239; Vol. 17, 1936, pp. 113–127.

[5] "A Study of the Weights and Dimensions of Trucks," *ibid.*, Vol. 16, 1935, pp. 37–52.

of empty trucks is between 0.35 and 0.40 of the total number of trucks. Studying the relation between the weight and length of trucks, it may be concluded that the ratios of the average weights of single trucks, tractor-semitrailers, and tractor-trailers are approximately 1:1.5:2.5 although the ratios of the respective lengths are about 1:1.5:1.9. Hence, the specific loads of single trucks very nearly equal those of tractor-semitrailer combinations; the tractor-trailer combinations are about one third heavier. The number of these combinations, however, does not exceed an average of 15% of the total number of trucks.

Distribution diagrams of the gross weight of both loaded and empty single trucks and of their over-all length have been deduced from the surveys and observations of the Public Roads Administration and presented in Fig. 1.

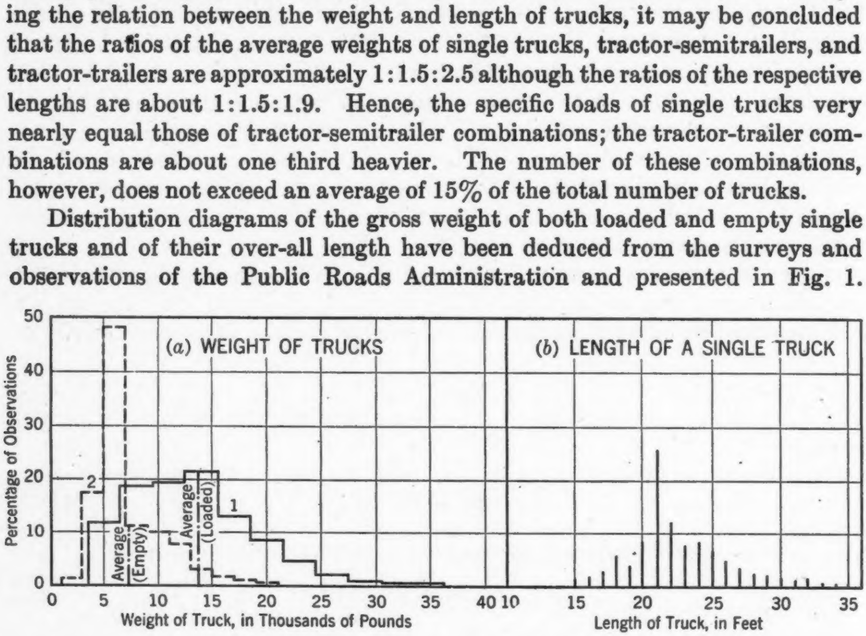


FIG. 1.—CURVES OF FREQUENCY DISTRIBUTION FOR THE PRINCIPAL TRAFFIC CHARACTERISTICS

Computations from the observations including values of σ and k give the following: From frequency distribution of weight (Fig. 1(*a*)):

Symbol	Curve 1 (loaded trucks)	Curve 2 (empty trucks)
$\bar{W}_1$	13,800 lb; $\bar{W}'_1$	7,200 lb
σ (standard deviation)	5,900 lb	3,200 lb
k (skewness)	+0.5	+1.5

$\bar{W}_1$ and W'_1 (maximum range, with a probability of 0.999)—

Computation	$W_1(\max) = \bar{W}'_1 + 3.5\sigma$	$W'_1(\max) = W'_1 + 3.9\sigma$
Result	34,500 lb	19,700 lb

and, from frequency distribution of length (Fig. 1(*b*)):

$$\bar{L} = 22.0 \text{ ft}$$
$$\sigma = 3.6 \text{ ft}$$
$$k = +\ 1.0$$

and the maximum range—

$$L\ (\max) = \bar{L} + 3.7\,\sigma = 35.5 \text{ ft (probability 0.999)}$$

No distribution diagrams are available for weights and lengths of passenger cars; their variations, however, are comparatively small and a specific load of 400 lb per ft of traffic lane with a range of fluctuation of some 25% may

be considered as representing actual conditions quite fairly. The average length of passenger cars may be assumed as 15 ft; no difference need be made between loaded and empty cars.

To establish the (average) design load according to Eq. 1, the frequency values in Table 1 should be introduced. These average frequency ratios deduced from observations, have been amended to provide for future development of traffic. The loads of trucks as presented in Fig. 1 are increased by 50% to anticipate rapid development of heavy cargo hauling on highways; likewise, the specific loads of passenger cars are increased by 20%, although even this figure appears excessive in view of the probable increased use of light materials in the construction of cars.

TABLE 1.—VALUES OF FREQUENCY PERCENTAGES OF GROUPS OF VEHICLES

Line	Traffic	F_V [a]	F_L [b]
1	Single trucks, buses, and tractor-semitrailers.....	0.25	0.70
2	Tractor-trailers..........	0.05	0.80

[a] Percentage of the total number (n) of vehicles.
[b] Average percentage of loaded vehicles in a group.

Introducing these modifications and considering the observed frequency distribution of load intensities as the "a priori" probability of the occurrence of these intensities in future—a procedure dating back to Aristotle who stated

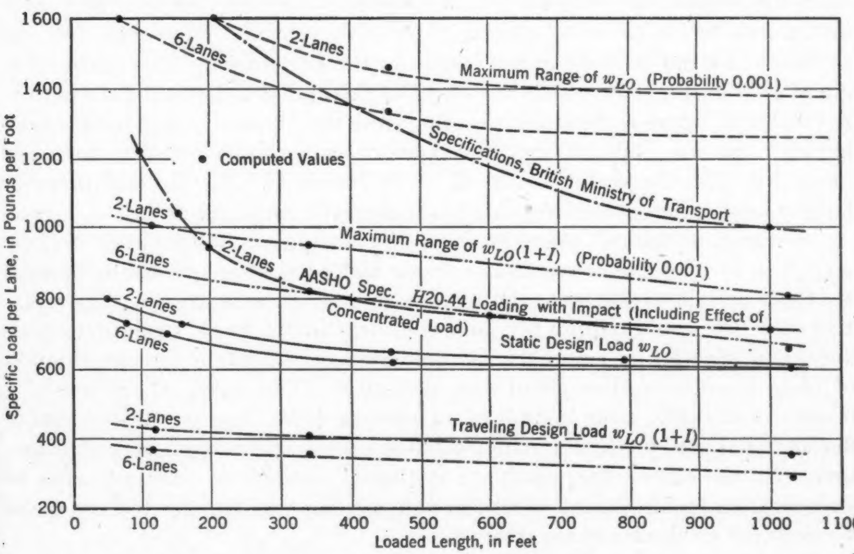


FIG. 2.—DESIGN LIVE LOADS FOR HIGHWAY BRIDGES

that "the probable is what for the most part happens"—the static design load w_{LO} and the "dynamic" design load $w_{LO}(1 + I)$ have been evaluated from Eqs. 1 to 4 and the frequency distributions of Fig. 1. The results are presented in Fig. 2. The individual average design values and the ranges of fluctuation pertaining to a probability of 0.001 that values will fall outside this range have

been computed for a two-lane structure and a six-lane structure of a length accommodating 3, 9, and 27 vehicles, respectively, per lane.

In establishing the maximum upper range of fluctuations, on which the safety factor pertaining to the design load depends, variations caused by fluctuations of the weight of vehicles according to Fig. 1(*a*), variations caused by fluctuations in the ratio of trucks, and variations caused by fluctuations in the ratio of empty vehicles have been evaluated individually and statistically superimposed. Thus, in the case of eighteen vehicles on the bridge, for instance, the static design load w_{LO} comprises one loaded tractor-trailer, four loaded trucks, and one empty truck, and twelve passenger cars, all of average weight; the maximum load due to fluctuations of weight results from the same grouping of vehicles of maximum weight. The maximum variation due to fluctuations of the ratio of trucks results in a group consisting of three loaded tractor-trailers and one empty tractor-trailer, six loaded trucks and two empty trucks, and six passenger cars, all of average weight; the range of variations due to fluctuations in the ratio of empty trucks is determined by replacing the empty truck in the design-load group by a loaded one. Different groupings of vehicles result in varying lengths of the structures accommodating them.

In Fig. 2 it is shown that, contrary to conventional assumptions, the static load w_{LO} considerably exceeds the "dynamic" load $w_{LO}(1 + I)$. This is because the influence of the dynamic load increment is less than the reduction of the load caused by the increased spacing of traveling vehicles. However, it should be borne in mind that the strain caused in the structure by the highly infrequent load w_{LO} is to be compared with the yield point or the static strength of the material, whereas the strain resulting from the frequently occurring traveling load—between 500,000 and 10,000,000 occurrences according to span and increasing with decreasing spans—is to be compared with the endurance or fatigue limit, which is considerably lower than the static strength.

A direct numerical comparison of the computed design loads w_{LO} or $w_{LO}(1 + I)$ with the conventional design loads would be misleading because the loads pertain to different safety factors and permissible stresses, the evaluation of which would require the joint consideration of loads, structural resistance, and selection of material—a procedure possible only if the presentation of facts is uniform throughout and statistical. The usual stipulation of a minimum strength value instead of an average value together with ranges of fluctuations based on the standard deviation effectively prevents such coordination. On the other hand, elaborate statistical methods of quality control of structural materials cannot affect the design if the traditional arbitrary specifications for service loads are retained.

However, even perfect coordination by systematic application of statistical methods will fall short of its purpose if it is one of form only. The importance of analyzing the compatibility of the correlated qualities cannot be emphasized strongly enough. There is as little sense in correlating the strain resulting from a frequently repeated, rapidly moving load with the yield limit of a material, as there is in correlating the strain produced by a steady or very slowly moving load with the fatigue limit.

Even without numerical comparison, however, it appears from Fig. 2 that the conventional values of design loads of highway bridges are substantially higher than could be justified from the factual evidence and a rather unfavorable appraisal of future developments.

Example 2. Fatigue Strength of Butt Welds in Ordinary Bridge Steel

To establish the "dependable" fatigue strength of butt welds in ordinary carbon bridge steel, the Committee on Fatigue Testing of the Welding Research Council of the Engineering Foundation has initiated an extensive program of tests of butt-welded specimens, prepared under fairly controlled commercial conditions.[6] From the test results, values are deduced for:

> "*** the unit fatigue strength which may be dependably expected in butt-welded joints commercially produced under standard specifications; to which dependable strength a designer will of course apply whatever factor of safety he deems appropriate for his particular condition."[7]

A study of the test results and of the conclusions shows that, because of the lack of a strictly rational, statistical interpretation of these results, their full implication has been missed, thus partly depriving an otherwise excellent and valuable test series of its merits. In the following independent analysis of the results the full information will be presented. This will lead to the establishment of a rational meaning of the term "dependable," as well as to an estimate of a more objective value of the safety factor than that which the designer "deems appropriate." Contrary to the procedure adopted in the Welding Research Council report, it has not been found necessary to introduce arbitrary criteria for eliminating certain test results only:

> "*** because they would in each case affect the average in a direction for which it is felt that they have too weak a basis in fact as compared with the other data in the same group."[8]

The results of tests on one hundred and eighty-six commercially fabricated, butt-welded specimens—5 in. by $\frac{7}{8}$ in. in critical cross section—are summarized in condensed form in Tables 2, 3 and 4. Data relative to the actual stress divided by the number of load cycles sustained (s_F/N), taken from individual tests have been converted into the approximate fatigue strength s_{FO} at N_o = 100,000 cycles and N_o = 2,000,000 cycles by using the empirical equation:

$$s_{FO} = s_F \left(\frac{N}{N_o}\right)^{\nu} \quad \text{(5)}$$

in a manner similar to that adopted in the report of the Welding Research Council.[6] Exponent ν was derived from tests by establishing the frequency distribution of the number of cycles N endured to failure under various stress

[6] "Fatigue Strength of Butt Welds in Ordinary Bridge Steel," *Report No. 3*, Committee on Fatigue Testing, Welding Research Council, New York, N. Y., 1943.

[7] *Ibid.*, p. 2.

[8] *Ibid.*, p. 6.

TABLE 2.—Observations to Determine the Fatigue Strength of Butt Welds (Specimens 5 In. by ⅞ In., Ordinary Structural Steel) Stressed From Zero to Tension

Test No.	N	Unit Strength[a] s_{FO}	
		N_e = 100,000	N_e = 2,000,000
(1)	(2)	(3)	(4)
(a) Applied Load from 0 to +30,000 Lb per Sq In.[a]			
Series X:			
1	202,600	35,400	17,500
2	220,700	36,100	17,900
3	591,200	45,500	22,500
Series Y:			
4	132,500	32,100	15,900
5	147,700	32,900	16,300
6	228,000	36,400	18,100
Series Z:			
7	112,800	30,800	15,400
8	123,000	31,500	15,600
9	134,500	32,100	15,900
Series XX:			
10	226,600	36,300	18,000
11	246,700	37,200	18,300
12	285,700	38,400	19,000
Series P:			
13	209,500	36,000	17,700
14	277,800	38,500	18,900
15	324,700	40,000	19,200
Series R:			
16	181,200	34,500	17,100
17	359,400	40,500	20,100
18	485,500	43,400	21,500
Series A:			
19	254,600	37,300	18,500
20	447,800	42,800	21,100
21	521,200	44,100	21,900
Series B:			
22	275,700	38,000	18,800
23	401,100	41,600	20,600
24	438,300	42,500	21,000
Series C:			
25	266,600	37,800	18,700
26	331,000	39,700	19,700
27	468,500	43,100	21,300
Series E:			
28	83,000	28,700	14,200
29	83,400	28,800	14,200
30	136,700	32,300	16,000
(b) Applied Load from 0 to +25,000 Lb per Sq In.[a]			
Series X:			
31	300,000	32,300	16,100
32	488,300	36,200	17,900
33	548,500	37,300	18,400
34	1,060,800	43,400	21,500
Series Y:			
35	603,500	38,100	18,900
36	631,700	38,500	19,100
37	678,300	39,200	19,400
Series Z:			
38	177,800	28,600	14,100
39	179,700	28,700	14,200
40	1,052,100	43,200	21,500
Series XX:			
41	984,400	42,700	21,100
42	1,103,200	43,900	21,600
43	1,154,900	44,500	22,000
(b).—(*Continued*)			
Series P:			
44	391,800	34,400	17,100
45	507,800	36,700	18,200
46	986,100	42,700	21,100
Series R:			
47	105,700	24,700	12,500
48	1,300,800	45,700	22,600
49	1,405,100	46,400	23,000
50	1,471,000	47,000	23,000
Series A:			
51	278,400	31,800	15,700
52	399,300	34,600	17,100
53	558,400	37,400	18,500
Series F:			
54	525,600	36,900	18,200
55	550,100	37,300	18,500
56	802,200	40,700	20,200
Series B:			
57	362,300	33,800	16,800
58	372,100	34,000	16,900
59	441,000	35,400	17,500
Series C:			
60	576,600	37,800	18,700
61	781,000	40,600	20,100
62	1,051,500	43,500	21,500
Series D:			
63	164,700	28,100	13,900
64	198,100	29,400	14,500
65	353,000	33,700	16,600
Series E:			
66	334,800	33,200	16,400
67	364,000	33,900	16,800
68	475,500	36,100	17,900
Series S:			
69	212,000	29,800	14,800
70	443,100	35,400	17,500
71	1,089,200	43,900	21,700
Series T:			
72	394,800	34,500	17,100
73	680,500	39,300	19,400
74	1,055,600	43,700	21,500
Series U:			
75	161,800	28,000	13,800
76	178,200	28,600	14,100
77	244,700	30,900	15,200
Series K:			
78	246,100	31,000	15,300
79	257,200	31,300	15,500
80	425,500	35,200	17,400
Series L:			
81	563,000	37,500	18,600
82	581,400	37,900	18,700
83	1,330,800	45,800	22,800
(c) Applied Load from 0 to +20,000 Lb per Sq In.[a]			
Series A:			
84	451,300	28,500	14,100
85	499,700	29,500	14,500
86	852,000	33,100	16,400

TABLE 2.—(*Continued*)

Test No. (1)	N (2)	Unit Strength[a] s_{FO}: N_e = 100,000 (3)	Unit Strength[a] s_{FO}: N_e = 2,000,000 (4)
(*c*).—(*Continued*)			
Series B:			
87	1,611,200	38,500	19,000
88	2,739,100	43,500	21,000
89	4,543,600	49,100	24,300
Series C:			
90	954,800	34,000	16,800
91	1,530,700	37,900	18,800
92	1,882,400	39,700	19,700
Series D:			
93	1,365,900	37,000	18,300
94	1,669,400	38,700	19,200
95	1,691,200	38,800	19,200
Series E:			
96	724,800	31,900	15,700
97	910,600	33,600	16,600
98	1,265,700	33,300	18,000
(*c*).—(*Continued*)			
Series U:			
99	1,548,300	38,200	18,800
100	4,354,400	48,700	24,000
Series K:			
101	574,600	30,100	14,900
102	811,000	32,700	16,200
103	1,228,000	36,100	17,800
Series L:			
104	1,305,300	36,500	18,100
105	1,508,600	37,900	18,700
106	2,609,900	42,900	21,300
Totals	....	3,912,350	1,934,950

[a] s_{FO} equals the fatigue strength, in pounds per square inch, computed from the applied load and the observed number of load applications sustained prior to failure. A positive sign denotes tension.

amplitudes (see Fig. 3) and computing the value of ν from the averages. This procedure is contrary to that of the Welding Research Council report in which a value of $\nu = 0.13$ was assumed on the basis of previous, and not entirely relevant, tests.

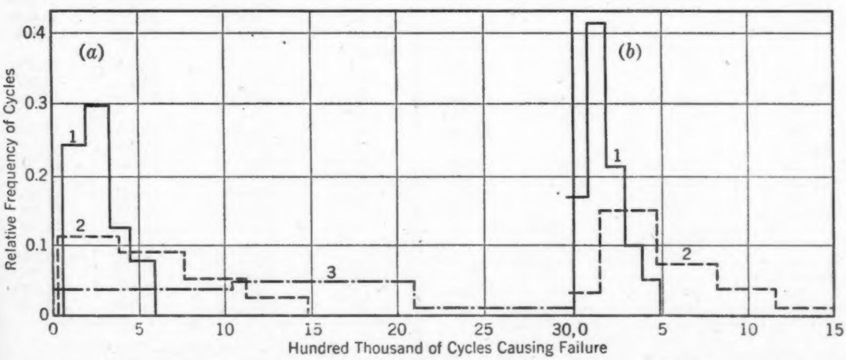


FIG. 3.—FREQUENCY DISTRIBUTION OF THE NUMBER OF CYCLES N REQUIRED TO CAUSE FAILURE

The applied procedure appears fully justified since the apparently inconsistent values of N show a unimodal distribution which, however widespread and skew, is fairly well reproducible by the second approximation of the Normal Law.[9] Thus, with the resulting values of $\nu = 0.235$ for pulsating stresses and $\nu = 0.205$ for reversing stresses, the approximate values of the fatigue strength

[9] *Manual on Presentation of Data*, A.S.T.M., 1937, pp. 24–27.

TABLE 3.—Observations to Determine the Fatigue Strength of Butt Welds (Specimens 5 In. by $\frac{7}{8}$ In., Ordinary Structural Steel) Under Full Stress Reversal

Test No.	N	Unit Strength[a] s_{FO}	
		$N_e =$ 100,000	$N_e =$ 2,000,000
(1)	(2)	(3)	(4)
(a) Applied Load from +20,000 to −20,000 Lb per Sq In.[a]:			
Series X:			
1	165,700	22,100	12,000
2	361,600	26,000	14,000
Series Y:			
3	84,700	19,300	10,400
4	184,000	22,600	12,300
5	329,900	25,500	13,800
Series Z:			
6	42,600	16,800	9,100
7	134,200	21,300	11,500
8	192,700	22,400	12,500
Series XX:			
9	279,500	24,700	13,400
10	391,500	26,400	14,300
11	402,700	26,600	14,400
Series P:			
12	42,300	16,800	9,000
13	67,200	18,500	8,900
14	78,200	19,000	10,200
Series R:			
15	83,900	19,300	10,400
16	143,600	21,500	11,600
17	434,700	27,000	14,700
Series F:			
18	160,100	22,000	11,900
19	161,500	22,100	11,900
20	197,300	23,000	12,500
Series G:			
21	68,000	18,500	10,000
22	119,200	20,800	11,300
23	132,200	21,200	11,500
Series S:			
24	85,300	19,400	10,500
25	225,900	23,700	12,800
26	238,400	23,900	13,000
Series T:			
27	152,700	21,800	11,800
28	188,100	22,800	12,300
29	305,100	25,100	13,600
Series L:			
30	104,200	21,800	10,900
31	245,300	24,000	13,000
32	261,300	24,300	13,200
(b) Applied Load from +19,000 To −19,000 Lb per Sq In.[a]			
Series XX:			
33	429,100	25,700	13,900
(c) Applied Load from +18,000 to −18,000 Lb per Sq In.[a]			
Series L:			
34	306,700	22,700	12,200
35	632,900	26,200	14,200
36	1,051,300	29,100	15,800
(d) Applied Load from +16,000 to −16,000 Lb per Sq In.[a]			
Series X:			
37	202,800	18,500	10,000
38	393,700	21,200	11,200
39	400,600	21,300	11,500
Series Y:			
40	737,500	24,100	13,100
(d).—(Continued)			
Series Y:			
41	833,500	24,700	13,400
42	1,254,200	26,800	14,600
Series Z:			
43	56,200	14,200	7,900
44	244,200	19,200	10,400
45	597,600	23,100	12,200
46	1,634,300	28,400	15,400
Series XX:			
47	263,000	19,500	10,600
48	336,800	20,500	11,100
49	545,500	22,700	12,300
Series P:			
50	118,000	16,600	9,000
51	197,000	18,400	10,000
52	305,100	20,100	10,900
Series R:			
53	83,400	15,400	8,300
54	85,600	15,500	8,400
55	148,200	17,800	9,400
56	935,900	25,300	13,700
Series D:			
57	80,700	16,700	8,200
58	155,800	17,500	9,500
59	246,000	19,400	10,400
Series F:			
60	504,400	22,300	12,100
61	603,500	23,100	12,500
62	616,300	23,200	12,600
Series G:			
63	345,300	20,600	11,200
64	727,100	24,000	13,000
65	1,006,000	25,600	13,900
Series S:			
66	613,000	23,200	12,600
67	733,900	24,100	13,000
68	805,800	24,500	13,300
Series T:			
69	300,700	20,800	11,300
70	440,200	21,700	11,700
71	1,059,400	25,900	14,000
Series U:			
72	452,000	21,800	11,800
73	470,000	22,000	11,900
74	955,200	25,400	13,800
Series L:			
75	214,100	18,700	10,100
76	359,900	20,800	11,300
77	530,000	22,500	12,200
78	663,000	25,900	12,800
(e) Applied Load from +15,000 to −15,000 lb per Sq In.[a]			
Series L:			
79	404,400	20,000	10,800
80	718,900	22,500	12,200
Totals	....	1,758,900	949,300

[a] s_{FO} equals the fatigue strength, in pounds per square inch, computed from the applied load and the observed number of load applications sustained prior to failure. A positive sign denotes tension.

TABLE 4.—FREQUENCY DISTRIBUTIONS OF FATIGUE STRENGTH, s_{FO}, TESTS IN TABLES 2 AND 3

Normal law	(a) Zero to Tension						(b) Full Reversal					
	N_o = 100,000			N_o = 2,000,000			N_o = 100,000			N_o = 2,000,000		
	Loading range (kips)	No.	Frequency	Loading range (kips)	No.	Frequency	Loading range (kips)	No.	Frequency	Loading range (kips)	No.	Frequency
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
5.4	23.9–29.1	9	8.5	12.05–14.55	11	10.4	14.10–17.26	7	8.8	7.55– 9.29	7	8.8
24.2	29.1–34.3	25	23.6	14.55–17.05	23	21.7	17.26–20.42	16	20.0	9.29–11.03	17	21.3
39.9	34.3–39.5	42	39.5	17.05–19.55	43	40.5	20.42–23.58	31	38.8	11.03–12.77	30	37.5
24.2	39.5–44.9	23	21.7	19.55–22.05	22	20.7	23.58–26.74	22	27.4	12.77–14.51	22	27.4
5.4	44.9–49.9	7	6.7	22.05–24.55	7	6.7	26.74–29.90	4	5.0	14.51–16.25	4	5.0

s_{FO} have been computed and presented in Tables 2 and 3. Statistical evaluation of the results and presentation of this fatigue strength for $N_o = 100{,}000$ cycles and $N_o = 2{,}000{,}000$ cycles in the form of a frequency distribution both for zero-to-tension amplitudes and for full-stress reversals (Tables 2 and 3 and Fig. 4) give the ranges of variation about the average values $\bar{s}_{FO} \pm C\,\sigma$ where

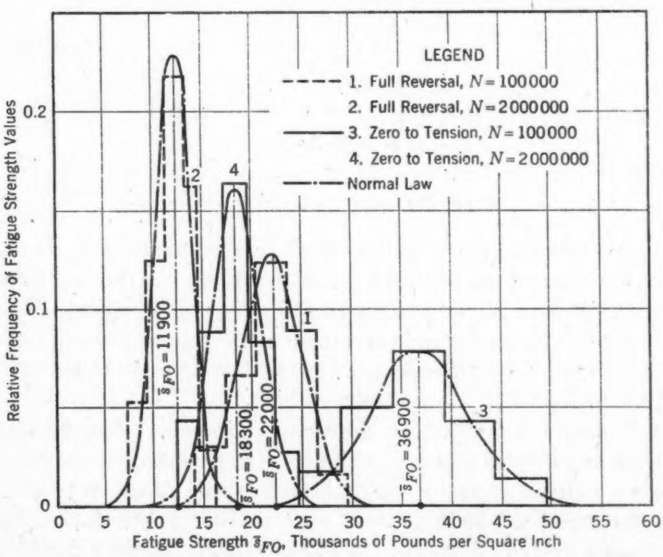


FIG. 4.—FREQUENCY DISTRIBUTION OF FATIGUE STRENGTH, S_{F}.

C denotes the range constants pertaining to selected probabilities of occurrence of values outside these ranges.

The routine computations have been omitted from Tables 2 and 3, and the following computations will serve as examples to demonstrate the procedure:

The last item of Col. 3, Table 2, is the total of the 106 values of s_{FO} computed from individual tests. The average value then is equal to 3,912,350/106 = 36,900 lb per sq in. A column of differences from the mean is then computed for each test, and another column for squared values. The sum of this last column being $\Sigma(s_{FO} - \bar{s}_{FO})^2 = 2{,}883.8 \times 10^6$, the standard deviation is $\sigma = \sqrt{\dfrac{2{,}883.8 \times 10^6}{106}} = \pm\ 5{,}200$ lb per sq in. To derive values of the skewness of the distribution k a third column $(s_{FO} - \bar{s}_{FO})^3$ is computed for each test and the total $\Sigma(s_{FO} - \bar{s}_{FO})^3$ is found to be $1{,}896.8 \times 10^9$. Thus, $k = \dfrac{(F - \bar{F})^3}{106 \times \sigma^3}$ $= k = \dfrac{1{,}896.8}{106 \times 5.2^3} = +\ 0.13$.

Similarly, for $N_o = 2{,}000{,}000$ cycles in Col. 4, Table 2: $\bar{s}_{FO} = 18{,}300$ lb per sq in., $\sigma = 2{,}500$ lb per sq in., and $k = +\ 0.09$; for $N_o = 100{,}000$ cycles (full reversal) in Col. 3, Table 3, $\bar{s}_{FO} = 22{,}000$ lb per sq in., $\sigma = 3{,}160$ lb per sq in., and $k = -\ 0.21$; and, for $N_o = 2{,}000{,}000$ cycles (full reversal) in Col. 4, Table 3, $s_{FO} = 11{,}900$ lb per sq in., $\sigma = 1{,}740$ lb per sq in., and $k = -\ 0.24$.

Referring to Fig. 4, computations for minimum strength ($C = 3.1$ pertaining to probability 0.001) give:

$$s_{FO1}\,(\text{min}) = 22{,}000 - 3.1 \times 3{,}160 = 12{,}200 \text{ lb per sq in.}$$
$$(100{,}000 \text{ full reversals})$$
$$s_{FO2}\,(\text{min}) = 11{,}900 - 3.1 \times 1{,}740 = 6{,}500 \text{ lb per sq in.}$$
$$(2{,}000{,}000 \text{ full reversals})$$
$$s_{FO3}\,(\text{min}) = 36{,}900 - 3.1 \times 5{,}200 = 20{,}800 \text{ lb per sq in.}$$
$$(100{,}000 \text{ zero to tension cycles})$$
$$s_{FO4}\,(\text{min}) = 18{,}300 - 3.1 \times 2{,}520 = 10{,}500 \text{ lb per sq in.}$$
$$(2{,}000{,}000 \text{ zero to tension cycles})$$

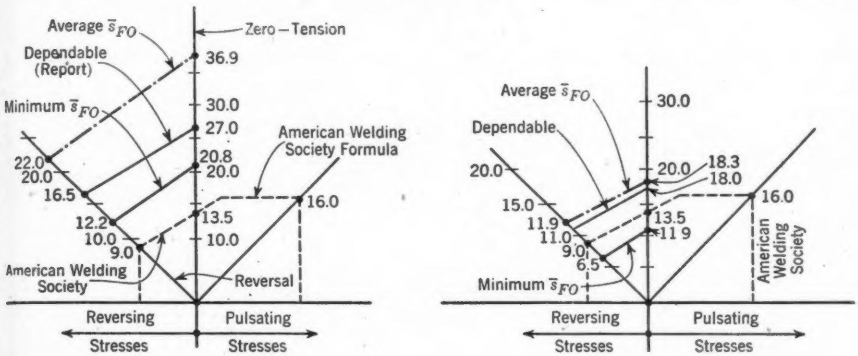
The form of the frequency distributions (Fig. 4) shows that, in spite of the apparent inconsistency of the primary test results, the final values are of a regularity typical of a genuine chance distribution as represented by the Normal Law. Even the considerable skewness of the distribution of the values of N has almost completely vanished, although not a single relevant result has been eliminated.

In Fig. 5 diagrams have been drawn of the average fatigue strength for various stress amplitudes and range of variation pertaining to a probability of 0.001 that an individual value will fall outside this range, together with the values recommended in the Welding Research Council report as "dependable" and with values derived from the American Welding Society Specification for Welded Bridges.[10] A comparison of all these values shows that the term "dependable" is meaningless unless it is classified by the appropriate probability.

To compare the ranges of variation of the fatigue strength of butt welds with those of the static strength s_T of the parent material, this strength has

[10] "Specifications for Welded Highways and Railway Bridges," Am. Welding Soc., 1941, p. 18, formula 7.

been computed, by statistical evaluation of the data contained in the Welding Research Council report,[6] as $s_T = 61{,}000 \pm 8{,}500 = 61{,}000\ (1 \pm 0.14)$. Ex-



(*a*) 100000 CYCLES (*b*) 2000000 CYCLES

FIG. 5.—FATIGUE STRENGTH AS A FUNCTION OF AMPLITUDE OF STRESS CYCLES, IN KIPS PER SQUARE INCH

pressed in terms of the average static strength the fatigue strength of butt welds has, therefore, the average values given in Table 5.

TABLE 5.—FATIGUE STRENGTH OF STATIC TENSILE STRENGTH s_T

Cycles	$N_e = 100{,}000$	$N_e = 2{,}000{,}000$
Zero to tension....	$0.61\ s_T$	$0.30\ s_T$
Full reversal......	$0.36\ s_T$	$0.195\ s_T$

The statistical difference between the specific maximum ranges of variation of the fatigue strength of butt welds about the average value (which is roughly $\pm$ 45%) and of the static strength of the parent material represents the range of variation caused entirely by the effects of welding and of fatigue. This range is therefore $\sqrt{(0.45)^2 - (0.14)^2} = \pm 43\%$. According to the definition of the objective safety factor,[2] it is required that in the design of butt welds in fatigue this factor be $\dfrac{1 - 0.14}{1 - 0.43} = 1.5$ times greater than the factor pertaining to the design of plain sections in static tension—if, all other things being equal, failure is to be prevented in both cases with equal probability.

CONCLUSION

The far-reaching effects of standard specifications in the fields of structural design and of production of engineering materials make it essential that in the preparation of such specifications the barrenness of conventionality and routine be evaded by the scientific attitude to engineering problems, and a spirit of vigorous critical analysis should replace traditional complacency. Nothing must be taken for granted and every relation assumed must be carefully examined by the most appropriate method, be it logical induction or statistical inference. Only thus will the impact of modern scientific development make itself felt in the engineer's everyday work.

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AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

PAPERS

DEVELOPMENT AND HYDRAULIC DESIGN, SAINT ANTHONY FALLS STILLING BASIN

By Fred W. Blaisdell,[1] Assoc. M. ASCE

Synopsis

Tests made to develop rules for the design of a stilling basin to dissipate hydrodynamic energy are described in this paper. The energy dissipator has been named the SAF stilling basin, SAF (denoting "Saint Anthony Falls") being a coined word used to differentiate this design from other stilling basin designs. In size, this stilling basin is the smallest known to the writer. The size is reduced through the use of baffles and sills within the stilling basin to assist in the dissipation of the energy of water flowing at high velocities.

The paper begins with a discussion of the reasons for making the tests reported and a brief discussion of previous work on energy dissipators. The scope of the test program is then discussed as are the apparatus and test methods used for each of the three groups or series of tests into which the study is arbitrarily divided. Each element involved in the design of the stilling basin is discussed separately in Section 6; under subheadings each of the various steps leading to the determination of the design equations is discussed; and the pertinent test data are summarized in tables and illustrations. The aim throughout has been to present the data in sufficient detail to permit an independent analysis by the reader so that he can arrive at his own conclusions and discuss the paper intelligently. The results are summarized for convenience of reference so that, after evaluating the design, the reader can obtain all the equations and information essential to the hydraulic design of the SAF stilling basin without "thumbing" through the entire paper.

Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by July 1, 1947.

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1. Introduction

Energy dissipators in some form have been made a part of many hydraulic structures throughout the world when the destructive energy in flowing water must be brought under control. Most of these energy dissipators have performed their intended function adequately—particularly those that have received the benefit of model tests. However, some of them have not performed their intended function, with the result that they have been destroyed or the structure of which they were a part has been damaged.

Except in unusual circumstances, only the larger structures are the subjects of individual model studies. However, they ordinarily pay dividends in improved performance and in reduced construction, maintenance, and possible replacement costs. In general, the structures built by the Soil Conservation Service (SCS), United States Department of Agriculture, are of such a size that few of them can justify, economically, the individual model studies that have proved so profitable for the larger structures. However, it becomes economical to make model studies if their costs can be spread over a large number of small structures, as is the case with the SCS where a large number of structures are required to further its soil and water conservation program. For use with certain of these structures it was essential that an efficient and economical stilling basin be developed and that design rules be formulated so that future stilling basins could be designed without recourse to further model studies. For this reason a study of methods of dissipating energy was instigated by the SCS in cooperation with the Minnesota Agricultural Experiment Station at the St. Anthony Falls Hydraulic Laboratory in Minneapolis, Minn.

2. Previous Work

The more important references consulted during the study upon which this paper is based are listed in the Bibliography (see Appendix I). The energy dissipators described in the references cited may be divided into three groups: Those that could be eliminated from further consideration as not being adaptable to the problem at hand (outlets for flumes or chutes and culverts), those that required critical study to evaluate them, and those that required tests to determine their potentialities.

No mention will be made of the first group. Of those that were classified in the second group, the simple stilling basin studied by C. Maxwell Stanley, M. ASCE (1),[2] is outstanding. This energy dissipator consists of a pool in which the energy in the high-velocity flow is dissipated. The pool is frequently formed by a sill or, in the case of high overflow dams, by a secondary low dam. Necessarily, the simple stilling basin is larger than the SAF basin described in this paper for comparable stilling action, since its design does not include the chute and floor blocks which dissipate much of the energy in the SAF basin. The elements and dimensions of the SAF stilling basin are defined in Fig. 1. The tests made to determine the best proportions of each element will be discussed separately in Section 6.

[2] Numerals in parentheses, thus: (1), refer to corresponding items in the Bibliography (Appendix I).

Exploratory tests were made on three types of stilling basin that were classified in the third group. The nappe-cutting energy dissipator studied at the Royal Technical University, in Stockholm, Sweden, is described by Wolmar

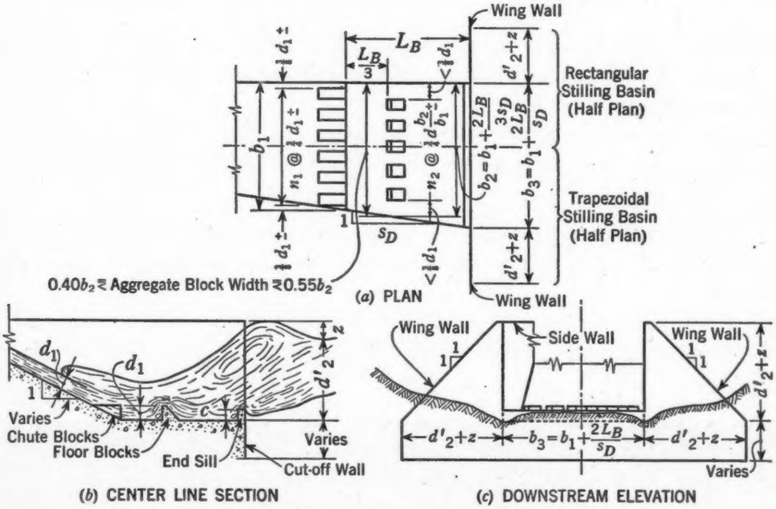


FIG. 1.—PROPORTIONS OF THE SAF STILLING BASIN

K. A. Fellenius (2a). This stilling basin was tested to check the claims made for it. The few tests at Minneapolis did not show any appreciable increase in the energy dissipation when a nappe cutter was used over the energy dissipation when no nappe cutter was used. As a result, no further consideration was given to this type.

Armin Schoklitsch (3a) has published results of experiments performed on an energy dissipator that is similar to Mr. Stanley's simple stilling basin except that a sill or bucket is used at the basin entrance. This sill causes the water to enter the stilling pool above its floor so that energy dissipation takes place both above and below the jet. A test with the sill removed, making the stilling basin similar to that tested by Mr. Stanley, showed that the use of the sill greatly increased the efficiency of the stilling basin as an energy dissipator. Mr. Schoklitsch has published charts giving the dimensions and performance of this stilling basin for different discharges and heights of dam. The tests on the Schoklitsch energy dissipator at Minneapolis were made after the SAF stilling basin studies were well advanced. At that time the SAF stilling basin was approximately one half of the size of the Schoklitsch dissipator that gave comparable results. Further experimentation on the Schoklitsch energy dissipator was abandoned in favor of the SAF stilling basin study.

Jacob E. Warnock, M. ASCE (4a), has described a rectangular stilling basin developed by the Bureau of Reclamation, United States Department of the Interior (USBR). Exploratory tests indicated that this basin was particularly efficient and that its size could be reduced considerably. As a result,

additional study was given to a stilling basin of this type, and from this study generalized design rules were formulated for proportioning the SAF stilling basin. Two general views of a stilling basin model are shown in Fig. 2.

3. Test Program

Notation.—The letter symbols used in the paper are defined where they first appear, in the text or by illustrations, and are assembled for convenience of reference in Appendix II.

The studies leading to the development of the SAF stilling basin are conveniently divided into three parts which are listed below in the chronological order of their performance:

(*1*) *The Culvert Outlet Series.*—In these tests the basin dimensions were first determined tentatively. The tests covered a narrow range of the Froude number, **F**.

(*2*) *The Flume Outlet Series.*—These tests covered a wide range of the Froude number and were made to expand the field of application of the SAF stilling basin.

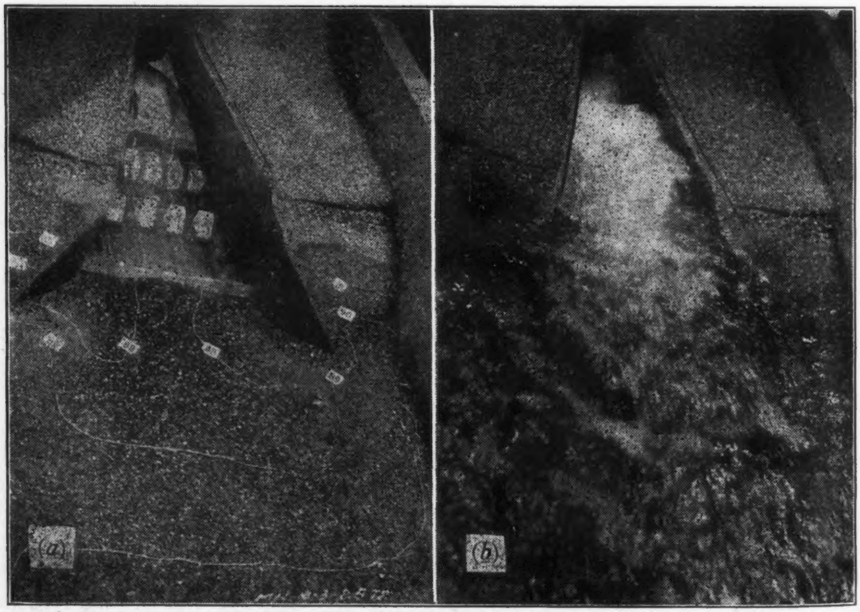


Fig. 2.—Model of SAF Stilling Basin

(*a*) Scour After Running Model 20 Hours; Basin Floor Elevation 88.0; Maximum Scour Elevation 87.4
(*b*) Design Discharge, Tailwater Elevation 93.0

(*3*) *The Turbine Room Series.*—The previous series of tests had shown the end sill height to be a function of the Reynolds number, **R**. The turbine room tests were made to extend the range of the Reynolds number, since its highest value for the culvert and flume outlet series was less than would be obtained

for most prototype structures, and to check the equation for the end sill height. The tests also covered a wide range of the Froude number.

The SAF stilling basin design was developed and verified as a result of two hundred seventy-four tests. The number of tests in each series and the range of the variables are given in Table 1. In Table 1, Q is the discharge; V_1 and

TABLE 1.—RANGE OF TEST VARIABLES

Series	Number of tests	Q (ft³/sec)		V_1 (ft/sec)		d_1 (ft)		d_2 (ft)		**F**		**R** $\times 10^{-3}$	
		From	To	From	To	From	To	From	To	From	To	From	To
Culvert outlet...	100	0.09	0.4	2.9	12	0.04	0.17	0.17	0.8	3	57	12.7	54
Flume outlet....	108	0.04	0.8	2.8	22	0.05	0.15	0.13	1.8	5	200	14.2	237
Turbine room...	66	0.40	21	9.7	44	0.03	1.27	0.49	5.5	7	288	40.6	2,100
Total...........	274	0.04	21	2.8	44	0.03	1.27	0.13	5.5	3	288	12.7	2,100

d_1 are the velocity and depth, respectively, at the entrance to the stilling basin; and d_2 is the theoretical depth after the hydraulic jump. The Froude number is defined by the equation:

$$\mathbf{F} = \frac{V^2_1}{g\,d_1} \quad \text{(1a)}$$

in which g is the acceleration due to the effect of gravity. The Reynolds number is defined by the equation:

$$\mathbf{R} = \frac{V_1\,d_1}{\nu} \quad \text{(1b)}$$

in which ν is the kinematic viscosity.

4. APPARATUS AND PROCEDURE

Both the culvert and flume outlet series of tests were made in a channel 8 ft long by 18 in. wide by 24 in. deep. Plate glass formed one side of the channel. Water was supplied to the models through a 4-in. pipe line and the rate of flow was controlled by a gate valve. The rate of flow through the model was determined by a calibrated 1-ft type HS-flume (5). Discharge measurements accurate to 1% were readily obtainable.

The channel in which the turbine room series of tests was made is shown in Fig. 3. A glass panel was placed in one side of this channel at the place where the stilling basin models were to be located. The rate of flow to the models was controlled by a gate valve in the 12-in. supply line. Three methods of measuring the flow were used. A calibrated 1.5-ft type H-flume (5) was used for discharges up to 5 cu ft per sec. For discharges greater than 5 cu ft per sec, the pressure drop across an 18-in. by 12-in. reducing flange was calibrated and used to determine the discharge until its calibration became invalid, probably because of rusting at the piezometers. A ½-in. Prandtl (6) pitot tube in the approach to the stilling basin model was then used for measuring the higher discharges.

Previous experiments on outlets (7) have shown that it is impossible to follow, visually, the progress of the erosion under the jet and "white water"

in and beyond the end of the stilling basin. Therefore, all models were split along their longitudinal center lines and one half of them were pressed against the glass side of the experimental channel. Check tests (8) showed that, within the limits of experimental error, the results obtained on the half model

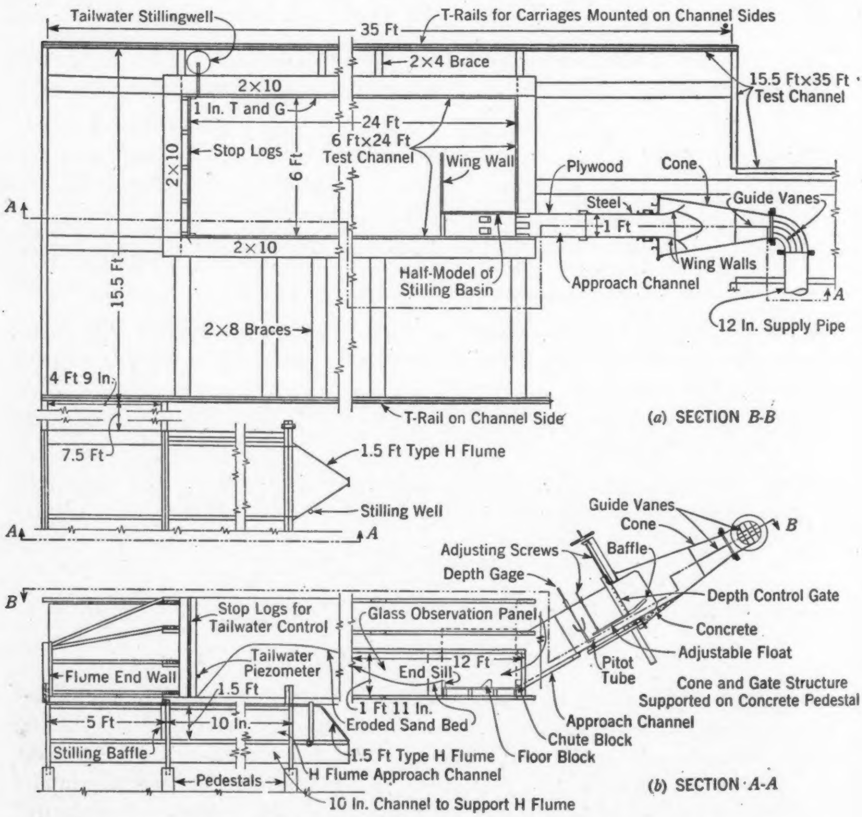


FIG. 3.—TEST APPARATUS, TURBINE ROOM SERIES

agreed with the full model. As a result all the tests reported in this paper were made on "half models" in which a glass plate was placed on the longitudinal center line.

For convenience, the approach to the stilling basin models for the culvert outlet series was considered to be a 3-ft square culvert built to a scale of 1 in. equals 1 ft. A half section of the culvert barrel, 1.5 in. by 3 in. in section and 7.5 ft long, was placed on a slope of 1% and connected to the 4-in. supply line. An open channel transition section was placed between the culvert and the stilling basin to spread out the water and drop it to the stilling basin floor level. The floor of the transition was given a parabolic shape. The width of the downstream end of the transition was varied to give the desired depth of flow at the stilling basin entrance. In designing the transition, the velocities in the culvert barrel and along the transition were assumed to be identical.

For the flume outlet series, a 3-in. square pipe, 7.5 ft long, was connected to the 4-in. supply line and a rectangular nozzle 3 in. wide with an adjustable lid was placed on a slope of 1 on 2 (a flatter slope was used for a few tests) and was used to control the depth of approach at the stilling basin entrance. The open channel between the nozzle and the stilling basin was 3 in. wide and its slope was 1 on 2.

The approach to the stilling basin model for the turbine room series is shown in Fig. 3. The gate for controlling the depth of flow at the entrance to the model is at the downstream end of the cone connected to the 12-in. supply line. As it left the gate, the stream was ordinarily well distributed across the channel leading to the model stilling basin. However, at the larger gate openings and at the larger discharges, the disturbances existing in the cone persisted and the depth distribution across the channel section was not satisfactory. Accordingly, an adjustable steel "float" was placed downstream from the gate to insure satisfactory flow conditions at the entrance to the stilling basin. This "float" was used for all tests in this series.

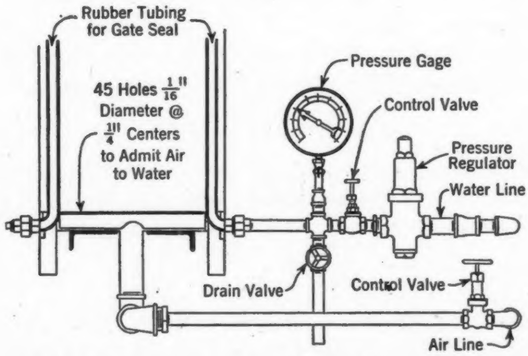


FIG. 4.—GATE SEAL AND AIR LINE DETAILS, TURBINE ROOM SERIES

The velocities obtained during the turbine room tests were such that the water would ordinarily entrain air. However, it was not possible to provide as long a channel between the depth control gate and the stilling basin as would have been desirable. For this reason there was insufficient distance for the water to entrain the air that would ordinarily be present in high-velocity flows. An arrangement for entraining air artificially was therefore incorporated in the design. A detail of this arrangement is shown in Fig. 4. The air flow was measured by a square-edged orifice using equations and coefficients published by H. S. Bean, E. Buckingham, and P. S. Murphy (9).

Stop logs were used in both test channels for tailwater control. Water levels and stream-bed contours were measured with the aid of point gages mounted on carriages in such a way that levels anywhere in the test channel could be obtained readily. These gages were also used in setting the models in their correct positions.

All models were of the "movable bed" type; the stream bed downstream from each stilling basin was formed in commercial concrete sand. The water

leaving the model stilling basin was permitted to erode the stream bed until the scour-hole dimensions had become stabilized. This action required about 30 min for the culvert and flume outlet series and 2 hours for the turbine room series. The size of the scour hole was taken as an indication of the effectiveness of the stilling basin (1*a*)(8).

The method of testing the models was as follows: The stilling basin was installed and the stream bed was filled with sand to an elevation higher than the anticipated elevation of the eroded stream bed. The stream bed was then flooded so that the initial rush of water through the stilling basin would not erode the stream bed excessively. The gate valve in the supply line was then opened to give the desired discharge and the tailwater level was adjusted to the correct depth. The discharge, tailwater depth, and water-surface profiles were recorded near the end of each test run. The stream bed was drained at the conclusion of each test. For the culvert and flume outlet series, the center-line profile of the eroded stream bed was recorded on data sheets. For the turbine room series, a contour map of the eroded stream bed was also obtained. Elevations of the stream bed at selected points near the stilling basin were also obtained and recorded.

Photographs were taken during many of the tests (see Fig. 2(*b*)). Motion pictures were also obtained for selected typical conditions.

5. Hydraulic Jump

All the SAF stilling basin dimensions are related either directly or indirectly to the hydraulic jump. The theoretical equation for the hydraulic jump is

$$d_2 = -\frac{d_1}{2} + \sqrt{\frac{2\,V^2_1\,d_1}{g} + \frac{d^2_1}{4}} \quad \ldots\ldots\ldots\ldots\ldots\ldots (2a)$$

The derivation of Eq. 2*a* can be found in most books on hydraulics (10a). Numerous experiments have proved the validity of this equation which can be simplified to

$$d_2 = \frac{d_1}{2}\left(-1 + \sqrt{8\,\mathbf{F} + 1}\right) \quad \ldots\ldots\ldots\ldots\ldots\ldots (2b)$$

6. Test Results

(*a*) *Length of Basin.*—As noted in Section 2, the SAF stilling basin was developed from the rectangular stilling basin described by Mr. Warnock (4*a*). The single rectangular stilling basin tested had a design length L_B of 3.75 d_2 although Mr. Warnock used 3.00 d_2. This basin was designed for a flow of Q = 275 cu ft per sec and a depth d_1 = 1.0 ft. The dimensions of this stilling basin (shown in Fig. 5) are in terms of the prototype, assuming the linear scale ratio to be 12 to 1. The results of the tests on this stilling basin under approximate design flow conditions are presented in Fig. 6(*a*) and Table 2, test C3. The profile shown in Fig. 6(*a*) is taken along the channel center line. Note that the toe of the roller is not at the upstream end of the basin. This test indicated that the stilling basin was much longer than was necessary to secure adequate energy dissipation, although it was entirely satisfactory from a purely hydraulic point of view.

TABLE 2.—Summary of Data for Tests to Determine Length of Basin

(Linear Scale Ratio, $L_r = 12$)

Test No.	Flow Q (ft³/sec) (1)	d_2 (ft) (2)	d'_2 (ft) (3)	L_B (ft) (4)	c (ft) (5)	F (6)	$\frac{d'_2}{d_2}$ (7)	$\frac{L_B}{d_2}$ (8)	$\frac{c}{d_2}$ (9)	Scour[a] (ft) (10)	Basin[b] (ft) (11)	z_s (ft) (12)	$\frac{z_s}{d_2}$ (13)	Performance[c] (14)
(a) Culvert Outlet Series C (d_1 = 1 Ft and b_1 = 9 Ft Except: In Test C75 d_1 = 2 Ft and b_1 = 4.5 Ft; and in Test C76 d_1 = 0.5 Ft and b_1 = 18 Ft)														
C63..	92	2.10	2.28	6.56	0.292	3.25	1.08	3.12	0.139	−1.0	−1.0	0.5	0.24	S
C64..	91	2.07	2.33	4.38	0.292	3.18	1.13	2.11	0.141	−1.0	−1.0	0.3	0.14	S
C56..	141	3.44	3.15	12.75	0.500	7.63	0.92	3.71	0.145	−2.4	−2.0	0.9	0.26	F
C57..	140	3.41	3.15	8.50	0.500	7.52	0.92	2.49	0.147	−2.1	−2.0	0.9	0.26	S−
C59..	140	3.41	3.20	9.88	0.500	7.52	0.94	2.90	0.147	−2.2	−2.0	0.9	0.26	F+
C60..	142	3.46	3.20	7.00	0.500	7.73	0.92	2.02	0.145	−2.3	−2.0	0.5	0.14	F
C75..	210	7.28	5.99	15.33	1.000	8.45	0.82	2.11	0.137	−4.8	−4.9	1.2	0.16	S
C71..	198	5.00	4.10	10.00	0.833	15.00	0.82	2.00	0.167	−3.7	−3.2	1.1	0.22	F
C72..	200	5.06	4.10	8.33	0.833	15.33	0.81	1.64	0.164	−3.7	−3.2	2.1	0.41	F
C76..	198	3.64	3.12	5.00	0.500	30.1	0.86	1.37	0.138	−2.7	−2.1	0.0	0.00	F
C3...	296	7.71	6.21	26.25	1.250	33.6	0.81	3.40	0.162	−5.2	−5.0	0.9	0.12	F+
C11..	288	7.49	6.15	20.00	1.250	31.8	0.82	2.67	0.167	−5.1	−5.0	0.3	0.04	S
C12..	283	7.35	6.21	17.50	1.000	30.7	0.84	2.38	0.136	−4.8	−5.0	0.1	0.01	S
C16..	282	7.32	6.09	14.00	1.000	30.5	0.83	1.91	0.136	−4.3	−5.0	1.1	0.15	G+
C19..	287	7.46	6.26	12.00	1.000	31.6	0.84	1.61	0.134	−4.4	−5.0	0.9	0.12	G
C22..	281	7.30	6.08	12.00	1.000	30.3	0.83	1.64	0.137	−4.2	−5.0	0.9	0.12	G
C85..	273	7.08	6.03	10.92	1.000	28.6	0.85	1.54	0.141	−4.1	−5.0	0.7	0.10	G
C28..	276	7.16	6.07	8.75	1.000	29.2	0.85	1.22	0.140	−4.5	−5.0	0.9	0.13	G−
C32..	275	7.13	6.08	7.00	1.000	29.0	0.85	0.98	0.140	−5.0	−5.0	2.0	0.28	F
C37..	278	7.20	6.06	5.00	1.000	29.6	0.84	0.69	0.139	−6.1	−5.0	2.9	0.40	P
C65..	386	10.19	9.80	10.00	1.417	57.0	0.96	0.98	0.139	−8.0	−8.5	1.4	0.14	G
C66..	375	9.90	9.83	8.33	1.417	53.9	0.99	0.84	0.143	−8.2	−8.5	1.9	0.19	G−
C67..	383	10.11	9.83	7.14	1.417	56.2	0.97	0.71	0.140	−8.5	−8.5	2.6	0.26	S
C68..	378	9.96	9.83	6.00	1.417	54.7	0.99	0.60	0.142	−8.7	−8.5	2.2	0.22	S−
C69..	379	10.00	9.83	5.00	1.417	55.0	0.98	0.50	0.142	−9.2	−8.5	2.8	0.28	F
C70..	378	9.96	9.83	4.00	1.417	54.7	0.99	0.40	0.142	−9.0	−8.5	1.9	0.19	F
(b) Flume Outlet Series F (d_1 = 1.20 Ft and b = 6 Ft Except: In Tests F52 and F57 d_1 = 1.03 Ft)														
F52..	200	7.62	6.47	14.02	1.021	31	0.85	1.84	0.134	+0.8	0.0	1.0	0.13	G
F57..	200	7.62	6.47	12.06	1.021	31	0.85	1.58	0.134	+0.5	0.0	1.2	0.16	G
F51..	284	10.15	8.63	14.21	0.762	40	0.85	1.40	0.075	+0.4	0.0	1.6	0.16	G
F55..	284	10.15	8.63	12.18	0.762	40	0.85	1.20	0.075	+0.7	0.0	1.9	0.19	G
F49..	375	13.61	11.45	16.33	1.021	70	0.84	1.20	0.075	+0.7	0.0	4.1	0.30	S−
F53..	375	13.61	11.45	13.61	1.021	70	0.84	1.00	0.075	+0.5	0.0	2.9	0.21	S−
F44..	375	13.61	11.41	10.88	1.021	70	0.84	0.80	0.075	+0.6	0.0	3.5	0.26	F
F48..	448	16.38	13.92	16.38	1.228	100	0.85	1.00	0.075	+0.9	0.0	4.2	0.26	S+
F54..	448	16.38	13.92	13.10	1.228	100	0.85	0.80	0.075	+1.0	0.0	4.7	0.29	S
F45..	448	16.38	13.92	9.83	1.228	100	0.85	0.60	0.075	+1.0	0.0	5.1	0.31	F
F46..	548	20.18	17.16	24.22	1.488	150	0.85	1.20	0.074	+1.0	0.0	3.9	0.19	G
F47..	548	20.18	17.16	20.18	1.488	150	0.85	1.00	0.074	+1.1	0.0	4.1	0.20	G
F50..	548	20.18	17.16	16.15	1.488	150	0.85	0.80	0.074	+1.1	0.0	4.7	0.23	G
F56..	548	20.18	17.16	12.11	1.488	150	0.85	0.60	0.074	+1.2	0.0	5.3	0.26	G

[a] Elevation of maximum depth of scour, in feet, on channel center line near end of stilling basin. [b] Elevation of the stilling basin floor. [c] Symbols denoting criteria for stilling basin performance, as follows:

G = good; maximum depth of scour only slightly below top of end sill and downstream bed level gradually rising as in Fig. 6(*c*).

S = satisfactory; no scour deeper than floor of stilling basin with the downstream bed level gradually rising.

F = fair; maximum depth of scour slightly below floor of stilling basin, or secondary scour holes, as in Figs. 6(*a*), 6(*f*), and 8(*d*) (structure not in danger of being undermined).

P = poor; maximum depth of scour considerably below floor of stilling basin and a large scour hole as in Fig. 6(*g*) (structure in danger of being undermined).

The next step in the investigation of length was to shorten the basin sufficiently to bring the toe of the roller back to the upstream end. The results of this shortening are presented in Fig. 6(*b*) and test C12, Table 2. The performance of the basin was not affected by this length reduction and, surprisingly, the maximum depth of the scour hole at the end of the stilling basin was reduced by 0.4 ft. Because these findings were encouraging, the basin length was reduced progressively with the results shown in Fig. 6 and Table 2.

The maximum depth of the scour hole decreased with a decrease in basin length until a length of 1.5 d_2 was reached. Further reduction in the basin

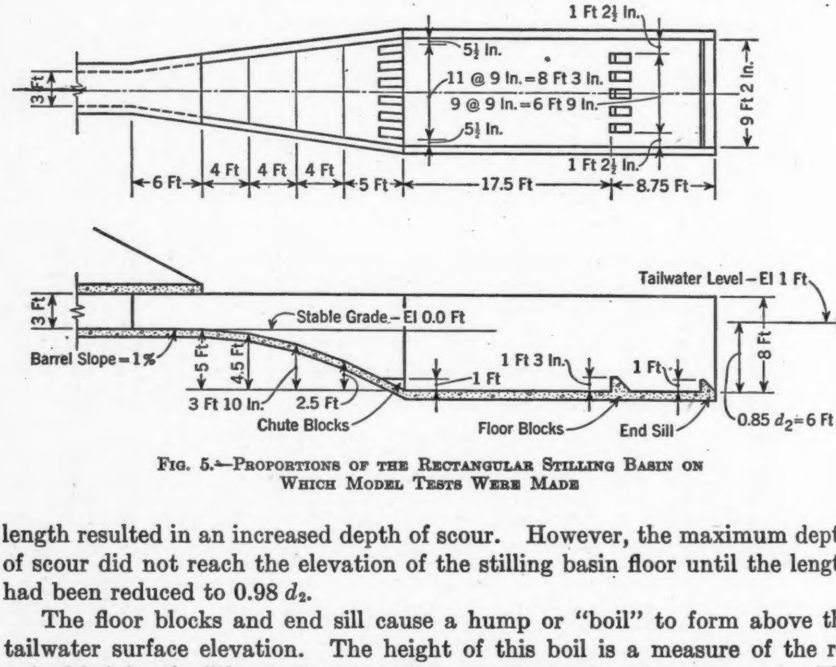


FIG. 5.—PROPORTIONS OF THE RECTANGULAR STILLING BASIN ON WHICH MODEL TESTS WERE MADE

length resulted in an increased depth of scour. However, the maximum depth of scour did not reach the elevation of the stilling basin floor until the length had been reduced to 0.98 d_2.

The floor blocks and end sill cause a hump or "boil" to form above the tailwater surface elevation. The height of this boil is a measure of the required height of stilling basin side wall if overtopping is to be prevented. The height of boil, determined for each test run, remained constant at about 1 ft above the tailwater surface elevation until the basin length was reduced to 0.98 d_2. For this length of basin the boil height increased to more than 2 ft. For $L_B = 0.69\, d_2$ the boil height reached 3 ft above the tailwater elevation. This increase in boil height required no higher side walls since the maximum boil height occurred downstream from the end of the stilling basin. However, the large size of the scour hole for $L_B = 0.98\, d_2$ was an indication that the energy dissipation was poor. A study of the data obtained from this series of tests resulted in the decision tentatively to select 1.25 d_2 as the best length of stilling basin.

After this length was selected, the study of basin lengths was deferred until the dimensions and spacing of the floor blocks and chute blocks, end sill height, and basin shape had been investigated and tentative conclusions had been

derived from the investigation. When the study of basin length was resumed, tests were planned to broaden the scope of the previous work. The principal dimensions of these basins and a summary of the results are given in Table 2.

As a result of these tests it was found, unfortunately, that the length of the stilling basin for discharges other than 275 cu ft per sec could not be predicted

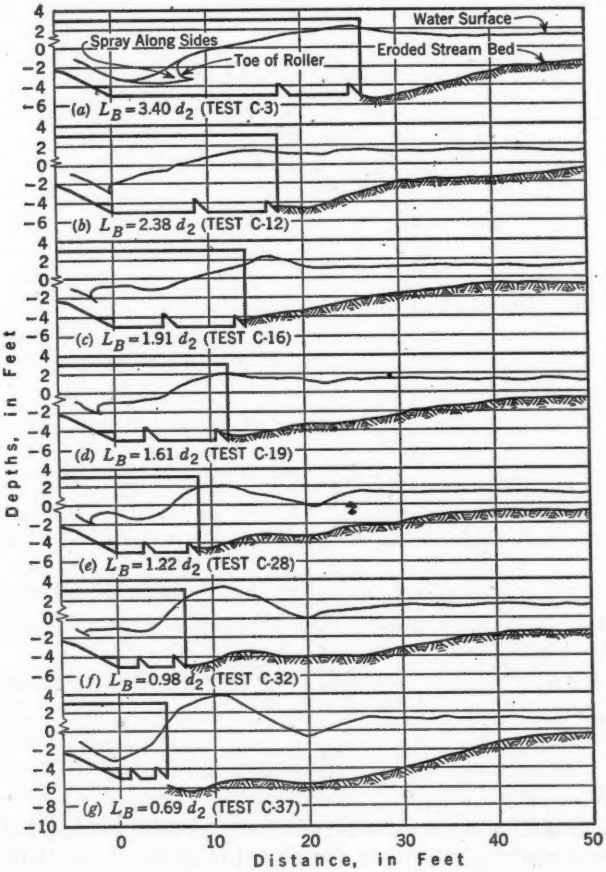


FIG. 6.—EFFECT OF LENGTH OF RECTANGULAR STILLING BASIN ON CENTER-LINE BED AND WATER-SURFACE PROFILES (SEE TABLE 2)

assuming that $L_B = 1.25\, d_2$. The stilling basins for smaller discharges were too short and those for higher discharges were longer than necessary. To determine the best length of basin for each discharge, L_B was decreased from run to run until the capability of the stilling basin to dissipate the energy became unsatisfactory.

After the data obtained for each test were studied, the composite performance of each length of stilling basin was placed in one of the four categories listed in Table 2, Col. 14, and the performances were plotted in Fig. 7. For

satisfactory performance the length of the stilling basin in terms of d_2 is shown to be considerably greater for the lower values of **F** than it is for the higher values of **F**. However, the data should be sufficient to permit a satisfactory determination of the basin length for all values of **F** between 3 and 150. In this connection it should be noted that a rating of "fair" was given to per-

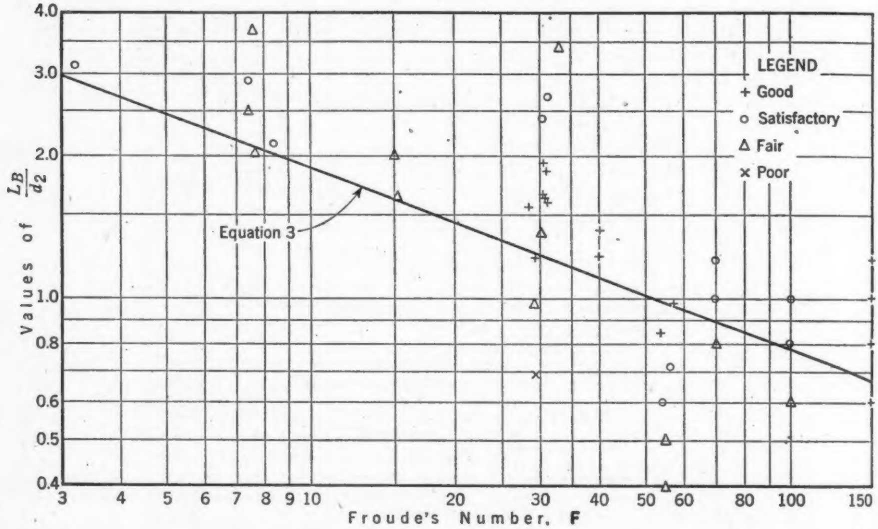


FIG. 7.—DIMENSIONLESS PLOT OF STILLING BASIN PERFORMANCE AS AFFECTED BY BASIN LENGTH

formances resulting in erosion that is not dangerous to the stability of an outlet when a short cutoff wall is used. A cutoff wall need not be used when the rating is "satisfactory."

A suggested curve to be used for design purposes in determining the length of the basin has been drawn in Fig. 7—that is:

$$\frac{L_B}{d_2} = \frac{4.5}{\mathbf{F}^{0.38}} \qquad (3)$$

Of course, there is some latitude in locating this curve. Eq. 3 is suggested as being conservative but not to the extent that the material in the outlet is wasted. Eq. 3 was developed for a range of the Froude number from 3 to 150 but was later used to design experimental stilling basins for the turbine room series having values of **F** as high as 288. Although no test in the turbine room series was designed specifically to check the equation for the length of the stilling basin, the results of the turbine room tests indicate that the original design equation is entirely satisfactory and that no change in either its form or constants is indicated.

(*b*) *Chute Blocks.*—The chute blocks at the entrance to the stilling basin serve to increase the effective depth of the entering stream, break it up into a number of small jets, and help to create the turbulence required for effective energy dissipation.

The height of the chute blocks was originally assumed to be d_1 and their width and spacing 0.75 d_1 at the downstream end. Their tops were made horizontal and their sides vertical. In plan they decreased in width with distance upstream to take into account the decrease in transition width (see Fig. 5). No change was made in the chute blocks until the basin dimensions had been tentatively selected and the floor blocks had been tentatively located. A test was then made on a solid chute block such as is used in the Schoklitsch energy dissipator (3*a*). Tests made on this change, when compared to tests made on the original chute block design, showed that there was less energy dissipated, that flow conditions in the channel downstream from the stilling basin were not as good, and that the height of the boil was greater. The chute blocks were next made 1.75 d_1 high at their downstream end and the top was sloped 1 on 3 so that the jet leaving the top of the chute blocks was directed at the base of the floor blocks. The result of this change was an increase in the depth of erosion near the end of the stilling basin over that obtained for the chute blocks originally used.

Chute blocks with the original dimensions were replaced on the transition. However, they were rectangular in plan instead of following the arrangement shown in Fig. 5; and their sides were placed parallel to the transition center line. Little difference was found between the rectangular and trapezoidal chute blocks—the advantage, if any, accruing to the rectangular blocks. The chute blocks used in all subsequent tests had a height of d_1 and a width and spacing of approximately 0.75 d_1. Throughout the tests made from Froude numbers of 3 to 288, no reason was found for making a change in these dimensions.

In making the subsequent tests, sometimes a block and sometimes a space were placed next to the side wall. Apparently, it makes no difference in the performance of the stilling basin whether a chute block or a space is next to the side wall as long as the blocks are symmetrical about the center line of the outlet.

(*c*) *Floor Blocks.*—The function of the floor blocks is to create the turbulence through which energy is dissipated. Energy is also removed from the water by impact against these blocks.

Longitudinal Position.—No particular attention was given to the location of the floor blocks during the first length-of-basin tests. After a length equal to 1.25 d_2 had been tentatively selected as the best, the longitudinal spacing of the floor blocks was investigated. The results of these tests are presented in Table 3(*a*) and Fig. 8. When the floor blocks were placed at a distance of $L_B/5$ from the upstream end of the basin (Table 3(*a*), test C41, and Fig. 8(*a*)), there was insufficient distance between them and the chute blocks to permit the turbulence caused by the chute blocks to become fully effective in dissipating energy. This condition is shown by the greater depth of erosion and slightly higher boil, for example, than in the case of test C28 in which the distance to the floor blocks is $L_B/3.2$ (Table 3(*a*), test C28, and Fig. 8(*b*)). On the other hand, when the distance to the floor blocks was 2 $L_B/3$ (Table 3(*a*), test C40, and Fig. 8(*d*)), there was insufficient distance between the floor blocks and the end sill to permit the turbulence caused by the floor blocks to become fully effective in dissipating the energy. This condition is also shown by the greater depth

and volume of erosion than for runs having a somewhat lesser distance to the floor blocks. The boil was higher than for any other run of this group because the floor blocks and end sill seemed to act together in deflecting the stream upward. The maximum depths of erosion for the two extreme floor block

TABLE 3.—SUMMARY OF DATA FOR TESTS MADE ON FLOOR BLOCKS

(Linear Scale Ratio, $L_r = 12$)

Test No.	Flow, Q (ft³/sec) (1)	d_2 (ft) (2)	d'_2 (ft) (3)	F (4)	$\frac{d'_2}{d_2}$ (5)	$\frac{L_B}{d_2}$ (6)	$\frac{c}{d_2}$ (7)	Scour[a] (ft) (8)	z_a (ft) (9)	$\frac{z_a}{d_2}$ (10)	Miscellaneous (11)
(*a*) LONGITUDINAL POSITION ($b_1 = 9$ FT; $d_1 = 1$ FT; $L_B = 8.75$ FT; $c = 1$ FT; AND BASIN FLOOR ELEVATION = −5 FT)											
C41...	276	7.17	6.09	29.3	0.85	1.22	0.14	−4.9	1.1	0.15	$L_B/5$[b]
C28...	276	7.17	6.07	29.3	0.85	1.22	0.14	−4.5	0.9	0.13	$L_B/3.2$[b]
C39...	272	7.04	6.09	28.3	0.87	1.24	0.14	−4.7	1.9	0.27	$L_B/2$[b]
C40...	282	7.32	6.09	30.4	0.83	1.19	0.14	−5.0	2.1	0.29	$2\ L_B/3$[b]
(*b*) HEIGHT ($b_1 = 9$ FT; $d_1 = 1$ FT; $L_B = 8.75$ FT; $c = 0.75$ FT; AND BASIN FLOOR ELEVATION = −5 FT)											
....	275	7.14	6.09	29.1	0.85	1.23	0.14	−5.0	1.9	0.27	$3\ d_1/2$[c]
C43...	276	7.17	6.09	29.3	0.85	1.22	0.14	−4.5	1.3	0.18	d_1[c]
C44...	274	7.09	6.07	28.7	0.86	1.23	0.14	−5.0	1.9	0.27	$3\ d_1/4$[c]
(*c*) ARRANGEMENT ($b_1 = 9$ FT; $d_1 = 1$ FT; $L_B = 8.75$ FT; $c = 1$ FT; AND BASIN FLOOR ELEVATION = −5 FT)											
C42...	281	7.29	6.10	30.2	0.84	1.20	0.14	−4.9	1.7	0.23	....[d]
(*d*) AGGREGATE WIDTH ($b_1 = 4.5$ FT; $d_1 = 2$ FT; $L_B = 15.33$ FT; $c = 1$ FT; AND BASIN FLOOR ELEVATION = −4.9 FT)											
C73...	202	6.96	6.03	7.8	0.87	2.20	0.14	−4.0	2.0	0.29	67[e]
C74...	199	6.86	5.99	7.6	0.87	2.23	0.15	−5.0	1.4	0.20	56[e]
C75...	210	7.26	5.99	8.4	0.83	2.11	0.14	−4.8	1.2	0.17	44[e]

[a] Elevation of maximum depth of scour, in feet, on the channel center line near the end of the stilling basin. [b] Distance from upstream end of stilling basin to floor blocks. [c] Height of floor blocks. [d] Floor blocks are in line with chute blocks. Compare with test C28 in which floor blocks are in line with spaces between chute blocks. [e] Percentage of stilling basin width occupied by floor blocks.

locations were approximately the same, indicating that it is as bad to have too little distance between the chute blocks and the floor blocks as it is to have the floor blocks and end sill too close together.

Both the depth of erosion and the scour pattern are nearly identical when the floor blocks are located $L_B/3.2$ and $L_B/2$ from the upstream end of the basin (Table 3(*a*), tests C28 and C39, and Figs. 8(*b*) and 8(*c*)), and it is impossible to state definitely which spacing is the better from the standpoint of scour. However, the boil is 1 ft higher for test C39 and therefore the longitudinal floor block spacing of $L_B/2$ was eliminated from further consideration.

The floor blocks were placed $L_B/3$ from the upstream end of the stilling basin for all subsequent tests. Although the subsequent tests covered a range of the Froude number from 3 to 288, no reason was discovered for changing the longitudinal floor block location.

Height.—The height of the floor blocks for the first length-of-basin tests was assumed to be 5 $d_1/4$. During these tests the floor block height was reduced to d_1 with no apparent adverse effects. When the floor block height was investigated, a further reduction to 3 $d_1/4$ was made. The result of this test (Table 3(*b*)) was an increase in both the height of boil and depth of scour.

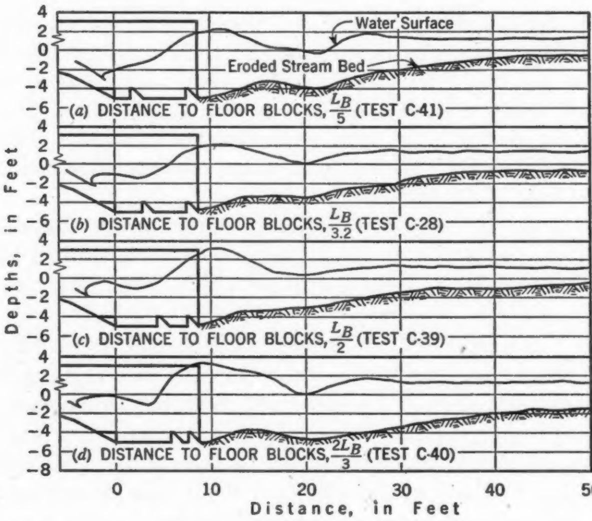


FIG. 8.—EFFECT OF FLOOR BLOCK LOCATIONS ON CENTER-LINE BED AND WATER-SURFACE PROFILES

The result of increasing the height of the floor blocks to 3 $d_1/2$ was the same—an increase in both the height of boil and depth of scour over that for a block height of d_1. Accordingly, a floor block height equal to d_1 was used for all subsequent tests.

Width and Spacing.—The width and spacing of the floor blocks was ordinarily made equal to those of the chute blocks. However, for the stilling basins that diverged in plan (see Fig. 1 and Section 6(*h*)), the floor block width and spacing were increased in proportion to the increase in stilling basin width at the floor block location.

Arrangement.—Ordinarily, the floor blocks were opposite the spaces between the chute blocks to intercept and break up the stream issuing through these spaces. To test the effectiveness of this arrangement, a single test was made with the floor blocks in line with the chute blocks. The results of this test (C42) are given in Table 3(*c*). They may be compared with the results of test C28, although the tabulated data do not give the complete story since a bad whirl developed along the side of the downstream channel for test C42 that caused considerable bed erosion and bank erosion. For all subsequent tests the floor blocks were alined with the spaces between the chute blocks.

Aggregate Floor Block Width.—Insufficient water can pass between the floor blocks if they occupy too much of the stilling basin width; they then act

more like a sill than like individual blocks. This fact was demonstrated vividly when a test was made on a narrow stilling basin with a higher value of d_1 than had been used prior to that time. The floor blocks, when $3\,d_1/4$ wide, obstructed 67% of the stilling basin width (Table 3(*d*), test C73). The boil caused by these wide blocks was very high and overtopped the basin side wall. (In all previous tests, where the floor blocks had obstructed 43% of the basin width, the boil height was much less.) The results of two additional tests on this stilling basin are given in Table 3(*d*). In these tests the floor block width was reduced to occupy 56% and 44% of the basin width, respectively, so that more water could flow around and between the floor blocks. The boil height was considerably reduced by these narrower floor blocks. The scour was slightly increased, but not sufficiently to cause concern since it did not extend lower than the basin floor level.

These tests indicate that the floor blocks should occupy between 40% and 55% of the stilling basin width. The aggregate width of all floor blocks, therefore, should be held within these limits even if it is necessary to reduce the width of the floor blocks to do so.

Transverse Position.—A floor block of full or half width was sometimes placed next to the stilling basin side wall. In each case this practice resulted in an increase in boil height at the side wall, requiring an increase in side-wall height to prevent overtopping. To avoid this, the criterion was established that no floor block should be placed closer to the stilling basin side wall than $3\,d_1/8$. The side-wall height (see Section 6(*f*)) was determined with this criterion in mind and no deviation from it should be permitted.

(*d*) *End Sill.*—The end sill, at the downstream end of the stilling basin (see Fig. 1), deflects the bottom currents upward and away from the stream bed. In addition, a ground roller is generated under the deflected jet. This ground roller transports bed material upstream and deposits it at the end of the stilling basin.

Originally, before much thought had been given to the problem, the end sill height was assumed to be d_1. This assumption proved satisfactory for the first runs of the culvert outlet series in which case Q, d_1, and d_2 were constant. Toward the end of series C (culvert outlet tests) a few tests were made on basins designed for indifferent values of Q and d_2 but with the same value of d_1. During these tests it was discovered that the end sill caused a high boil when d_2 was low and the end sill blocked off a large part of the stilling basin exit area—that is, when the ratio of end sill height c to d_2 was high. The height of the end sill was therefore made proportional to d_2 rather than to d_1. A little thought shows this to be more logical. The function of the end sill is to deflect the bottom currents upward and in doing so, the entire stream leaving the stilling basin is deflected. To have the same degree of deflection for all depths of water at the end of the basin, the height of the end sill should vary with d_2. The satisfactory height of end sill used in the first few tests was (in terms of d_2) equal to $d_2/7$. Therefore, this end sill height was used with entirely satisfactory results until the tests of the flume outlet series, which were made at higher values of **F**, showed that further changes were required.

The design conditions and results of the flume outlet tests made to determine the best end sill height are summarized in Table 4. The tests were made by holding all variables constant for each Froude number with the exception of

TABLE 4.—SUMMARY OF DATA FOR TESTS TO DETERMINE HEIGHT OF END SILL; FLUME OUTLET SERIES F

($L_r = 12$; $b = 6$ Ft; and $d'_2/d_2 = 0.85$)

Test No.	c (ft) (1)	$\frac{c}{d_2}$ (2)	Scour[a] (ft) (3)	z_a (ft) (4)	$\frac{z_a}{d_2}$ (5)	Remarks (6)
(a) $Q = 100$ Cu Ft per Sec; $d_1 = 0.6$ Ft; $d_2 = 5.1$ Ft; $d'_2 = 4.3$ Ft; $L_B = 6.1$ Ft; $L_B/d_2 = 1.20$; $F = 40$; and $R_m \times 10^{-3} = 30.1$						
F72....	0.25	0.0490	−0.4	0.3	0.06	....
F70....	0.50	0.0980	+0.1	0.9	0.18	....
F71....	0.76	0.150	+0.5	1.0	0.20	Best height of end sill.
F73....	1.01	0.198	+1.0	1.7	0.33	....
(b) $Q = 637$ Cu Ft per Sec; $d_1 = 1.8$ Ft; $d_2 = 18.8$ Ft; $d'_2 = 16.2$ Ft; $L_B = 14.3$ Ft; $L_B/d_2 = 0.76$; $F = 60$; and $R_m \times 10^{-3} = 191.7$						
F81....	0.95	0.050	0.0	1.8	0.10	Best height of end sill.
F80....	1.91	0.102	+1.3	5.0	0.27	High boil.
F82....	2.86	0.152	+2.2	6.9	0.37	High boil.
(c) $Q = 374$ Cu Ft per Sec; $d_1 = 1.2$ Ft; $d_2 = 13.6$ Ft; $d'_2 = 11.4$ Ft; $L_B = 10.2$ Ft; $L_B/d_2 = 0.75$; $F = 70$; and $R_m \times 10^{-3} = 123.0$						
F60....	0.68	0.050	−0.5	2.3	0.17	....
F58....	1.02	0.075	+0.7	3.3	0.24	Best height of end sill.
F59....	1.36	0.100	+1.0	4.5	0.33	Second hole.
(d) $Q = 158$ Cu Ft per Sec; $d_1 = 0.6$ Ft; $d_2 = 8.2$ Ft; $d'_2 = 7.0$ Ft; $L_B = 6.1$ Ft; $L_B/d_2 = 0.75$; $F = 100$; and $R_m \times 10^{-3} = 47.7$						
F76....	0.41	0.050	−0.1	0.6	0.07	....
F75....	0.82	0.100	+0.4	1.9	0.23	Best height of end sill.
F74....	1.22	0.149	+1.0	3.2	0.39	Second hole.
(e) $Q = 448$ Cu Ft per Sec; $d_1 = 1.2$ Ft; $d_2 = 16.4$ Ft; $d'_2 = 13.9$ Ft; $L_B = 12.3$ Ft; $L_B/d_2 = 0.75$; $F = 100$; and $R_m \times 10^{-3} = 138.5$						
F66....	0.82	0.050	−0.1	1.6	0.10	Sill heights equally good; depth of scour greater for test F66 but boil height greater for test F65.
F65....	1.23	0.075	+1.0	3.6	0.22	
F64....	1.63	0.099	+1.1	5.1	0.31	Second hole.
(f) $Q = 549$ Cu Ft per Sec; $d_1 = 1.2$ Ft; $d_2 = 20.2$ Ft; $d'_2 = 17.2$ Ft; $L_B = 15.1$ Ft; $L_B/d_2 = 0.75$; $F = 150$; and $R_m \times 10^{-3} = 165.4$						
F62....	1.01	0.050	0.0	2.8	0.14	Height of end sill satisfactory.
F63....	1.49	0.074	+1.1	5.0	0.25	Best height of end sill.
F61....	2.02	0.100	+1.6	8.0	0.40	High boil.

[a] Elevation of maximum depth of scour on the channel center line, in feet, near the end of the stilling basin. The basin floor is at El. 0.

the height of end sill. Tests were made at that end sill height which, in the judgment of the observer, would give the best results. Additional tests were made using higher and lower end sills to check the assumed value of c.

The plotted center-line profiles of the water surface and the eroded sand beds were studied to determine the best end sill height. The height judged best on this basis is noted in Table 4. Ordinarily, a low end sill did not deflect the jet upward sufficiently to prevent stream-bed erosion close to the outlet and to permit the formation of a ground roller under the jet. On the other hand, a high end sill caused the jet to shoot into the air. Thus, a higher side wall was required ordinarily and the landing stream eroded a large hole some distance downstream from the stilling basin. In Table 4, the terms "second hole" and "high boil" indicate the same condition; because of the test setup the second hole did not always appear and the term "high boil" is substituted as a synonomous term.

The flume outlet series was completed by making tests to check the over-all performance of the stilling basin. The results of these tests are summarized in Table 5. The end sill heights used in designing the stilling basins for these tests were found to be entirely satisfactory.

Considerable study was given to the problem of reducing the end sill height observations of the culvert and flume outlet series to a single curve before a successful solution was obtained. Plotting c/d_2 versus $\mathbf{F}$ did not reduce the data to such a form that a single curve could be used. Studies of this plot indicated that the variation was a function of the discharge per unit width of stilling basin. Plotting the product of c/d_2 and the discharge per unit width versus $\mathbf{F}$ produced a very satisfactory curve which was used in designing end sills for the tests listed in Table 5. The equation of this curve was not dimensionless, and it was therefore necessary to devise a method of making the equation generally applicable. Although two methods were used to accomplish this result, only the simpler and most straightforward method will be described. In the Reynolds number, defined in Eq. 1*b*, the term $V_1 d_1$ is the discharge per unit width. The kinematic viscosity, ν, can be considered constant for design purposes. For this condition the Reynolds number is proportional to the discharge per unit width. Since $\mathbf{R}$ is dimensionless, it does not have the undesirable dimensional properties of the equation first developed for the height of end sill. A consideration of the previously developed equation indicated that $\mathbf{F}$ had little influence on the determination of the end sill height. Therefore, the Froude number was tentatively eliminated from further consideration and c/d_2 was plotted against $\mathbf{R}$. The curve drawn through these data was quite well defined and had the equation,

$$\frac{c}{d_2} = \frac{24}{\sqrt{\mathbf{R}}} \qquad (4)$$

Unfortunately, the experimental data did not cover the complete range of Reynolds numbers that may be expected in designing field structures—because the models were operated in accordance with the Froude model law (gravitational forces controlling) whereas the prototype-model relationship for the end sill height developed from the culvert and flume outlet series of tests requires that the model be operated in accordance with the Reynolds model law (viscous forces controlling). Since the two model laws require different and incom-

patible operating conditions, it is not possible to operate a model in such a way as to fulfil the requirements of both laws simultaneously.

The end sill heights computed from Eq. 4 are unbelievably low for the Reynolds numbers within the range of practical application. This condition

TABLE 5.—Summary of Data for Tests to Check Height of End Sill; Flume Outlet Series F

(L_r = 12 and b = 6.0 Ft)

Test No.	Flow Q (ft³/sec) (1)	d_1 (ft) (2)	d_2 (ft) (3)	d'_2 (ft) (4)	L_B (ft) (5)	c (ft) (6)	$R_m \times 10^{-3}$ (7)	$\frac{d'_2}{d_2}$ (8)	$\frac{L_B}{d_2}$ (9)	$\frac{c}{d_2}$ (10)	Scour* (ft) (11)	z_a (ft) (12)	$\frac{z_a}{d_2}$ (13)
(*a*) Froude Number F = 5													
F103...	36	0.6	1.6	1.7	4.0	0.38	14.1	1.06	2.50	0.237	0.0	0.7	0.44
F108...	101	1.2	3.2	3.4	7.9	0.46	39.9	1.06	2.47	0.141	+0.1	0.5	0.16
F105...	184	1.8	4.9	5.1	11.9	0.50	72.9	1.04	2.43	0.102	+0.2	0.8	0.16
(*b*) Froude Number F = 10													
F107...	142	1.2	4.8	4.9	9.0	0.50	56.1	1.02	1.88	0.105	+0.1	0.5	0.10
F104...	260	1.8	7.2	7.3	13.5	0.55	103.2	1.02	1.88	0.077	+0.4	0.3	0.04
(*c*) Froude Number F = 20													
F106...	201	1.2	7.0	6.5	10.1	0.58	79.5	0.93	1.44	0.082	0.0	0.5	0.07
(*d*) Froude Number F = 30													
F89....	86	0.6	4.4	3.7	5.5	0.54	30.8	0.85	1.25	0.125	+0.3	1.2	0.27
F92....	246	1.2	8.7	7.4	11.0	0.64	93.3	0.85	1.26	0.073	0.0	1.1	0.10
F93....	246	1.2	8.7	7.4	11.0	0.64	97.3	0.85	1.26	0.073	+0.1	0.8	0.09
F91....	450	1.8	13.1	11.1	16.5	0.71	160.7	0.85	1.26	0.054	+0.5	0.9	0.07
(*e*) Froude Number F = 60													
F87....	123	0.6	6.3	5.3	6.0	0.70	43.8	0.85	0.95	0.111	+0.2	1.7	0.27
F85....	347	1.2	12.5	10.7	11.9	0.82	124.1	0.85	0.95	0.065	0.0	1.3	0.10
F90....	637	1.8	18.8	16.0	17.9	0.91	227.7	0.85	0.95	0.048	−0.1	1.0	0.05
(*f*) Froude Number F = 100													
F88....	159	0.6	8.2	7.0	6.4	0.90	56.7	0.85	0.78	0.110	0.0	2.0	0.24
F84....	448	1.2	16.4	13.9	12.8	1.06	160.7	0.85	0.78	0.065	+1.0	4.1	0.25
(*g*) Froude Number F = 150													
F83....	549	1.2	20.2	17.2	13.5	1.36	196.4	0.85	0.67	0.067	+1.1	2.8	0.14

*Elevation of maximum depth of scour on the center line of the channel, in feet, near the end of the stilling basin. The basin floor is at El. 0.

led to the suspicion that the form of Eq. 4 might be incorrect, although nothing occurred during the culvert and flume outlet series to indicate that Eq. 4 could not be safely extrapolated, and showed the advisability of making additional tests at higher values of **R**. The turbine room series of tests was designed

TABLE 6.—SUMMARY OF DATA, TURBI

Test No.	Flow, Q (ft^3/sec)	V_1 (ft/sec)	d_1 (ft)	d_2 (ft)	d'_2 (ft)	L_B (ft)	c (ft)	**F**	**R** $\times 10^{-3}$	$\frac{d'_2}{d_2}$	$\frac{c}{d_2}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
T38......	0.408	13.6	0.03	0.572	0.419	0.348	0.067	191	41.2	0.732	0.117
T39......	0.399	13.3	0.03	0.560	0.473	0.348	0.039	183.1	40.6	0.845	0.070
T37......	0.408	10.2	0.04	0.489	0.427	0.413	0.058	80.8	43.4	0.874	0.118
T46......	1.05	21.0	0.05	1.146	0.958	0.602	0.076	274	98.6	0.835	0.066
T47......	1.06	20.7	0.111	1.142	0.945	0.602	0.076	120	....	0.829	0.066
T44......	1.06	17.7	0.06	1.050	0.883	0.672	0.069	161	99.2	0.840	0.066
T45......	1.02	16.9	0.102	1.005	0.864	0.672	0.069	87.0	....	0.860	0.069
T42......	1.016	14.5	0.07	0.924	0.774	0.736	0.064	93.7	104.2	0.838	0.069
T43......	1.010	14.5	0.094	0.919	0.775	0.736	0.064	69.2	....	0.844	0.070
T40......	0.970	9.7	0.10	0.716	0.629	0.900	0.054	29.2	98.6	0.878	0.075
T35......	2.02	25.2	0.08	1.740	1.433	0.961	0.100	247	212	0.823	0.057
T36......	1.99	24.9	0.08	1.715	1.432	0.961	0.150	240	214	0.835	0.087
T33......	2.00	20.0	0.10	1.525	1.327	1.098	0.191	124	204	0.870	0.125
T34......	1.98	19.8	0.10	1.510	1.300	1.098	0.100	121.7	201	0.861	0.066
T29......	2.01	16.8	0.12	1.386	1.167	1.223	0.191	72.6	191	0.841	0.138
T30......	1.99	16.6	0.12	1.373	1.173	1.223	0.100	71.2	192	0.854	0.073
T31......	1.99	16.6	0.12	1.373	1.182	1.223	0.070	71.2	197	0.861	0.051
T32......	2.00	16.7	0.144	1.380	1.187	1.223	0.070	60.0	....	0.860	0.051
T25......	1.95	13.0	0.15	1.182	1.051	1.388	0.290	35.0	206	0.889	0.245
T26......	2.00	13.3	0.15	1.212	1.063	1.388	0.191	36.8	208	0.876	0.158
T27......	2.01	13.4	0.15	1.221	1.075	1.388	0.100	37.2	200	0.881	0.082
T28......	1.99	13.3	0.15	1.207	1.075	1.388	0.062	36.4	197	0.890	0.051
T21......	1.94	9.7	0.20	0.985	1.017	1.622	0.290	14.6	198	1.032	0.294
T22......	2.02	10.1	0.20	1.030	1.049	1.622	0.191	15.8	209	1.018	0.185
T23......	2.01	10.0	0.20	1.020	1.032	1.622	0.100	15.5	214	1.011	0.098
T24......	2.01	10.0	0.20	1.020	1.040	1.622	0.062	15.5	220	1.019	0.061
T53......	4.00	33.3	0.12	2.82	2.397	1.472	0.197	288	426	0.850	0.070
T50......	4.00	25.0	0.16	2.410	2.053	1.754	0.169	121	404	0.852	0.070
T51......	4.00	25.0	0.16	2.410	2.070	1.754	0.169	121	407	0.859	0.070
T52......	4.00	25.0	0.16	2.410	2.070	1.754	0.169	121	417	0.859	0.070
T48......	4.50	22.5	0.20	2.405	1.820	1.986	0.148	78.5	435	0.757	0.062
T49......	4.50	22.2	0.223	2.398	1.924	1.986	0.148	68.7	....	0.803	0.062
T41......	4.00	16.0	0.25	1.871	1.597	2.271	0.131	31.8	408	0.853	0.070
T13......	7.00	38.9	0.18	4.023	3.408	2.187	0.112	261	730	0.847	0.027
T14......	7.00	38.9	0.18	4.023	2.656	2.187	0.192	261	707	0.660	0.047
T15......	7.00	38.9	0.18	4.023	3.005	2.187	0.192	261	715	0.747	0.047
T16......	7.00	38.9	0.18	4.023	3.033	2.187	0.305	261	677	0.754	0.075
T17......	7.00	38.9	0.18	4.023	3.430	2.187	0.305	261	690	0.853	0.075
T5......	7.00	35.0	0.20	3.800	2.70	2.329	0.106	190	752	0.710	0.027
T6......	7.00	35.0	0.20	3.800	2.715	2.329	0.190	190	750	0.715	0.050
T7......	7.00	35.0	0.20	3.800	2.746	2.329	0.266	190	756	0.722	0.070
T8......	7.00	35.0	0.20	3.800	3.19	2.329	0.266	190	750	0.839	0.070
T9......	7.00	35.0	0.20	3.800	3.36	2.329	0.106	190	760	0.884	0.027
T1......	7.00	28.0	0.25	3.359	2.856	2.656	0.099	97	686	0.850	0.029
T2......	7.00	28.0	0.25	3.359	2.62	2.656	0.169	97	730	0.780	0.050
T3......	7.00	28.0	0.25	3.359	2.65	2.656	0.235	97	745	0.789	0.070
T4......	7.00	28.0	0.25	3.359	2.67	2.656	0.336	97	753	0.795	0.100
T18......	7.00	21.2	0.33	2.875	2.45	3.119	0.146	42.3	716	0.852	0.051
T19......	7.00	21.2	0.33	2.875	2.454	3.119	0.204	42.3	716	0.854	0.071
T20......	7.00	21.2	0.33	2.875	2.477	3.119	0.292	42.3	718	0.862	0.102
T10......	7.00	14.0	0.50	2.232	2.25	3.882	0.062	12.2	745	1.008	0.027
T11......	7.00	14.0	0.50	2.232	2.225	3.882	0.250	12.2	730	0.997	0.112
T12......	7.00	14.0	0.50	2.232	2.232	3.882	0.500	12.2	740	1.000	0.224
T62......	9.70	43.7	0.222	5.02	4.345	2.720	0.360	267	800	0.865	0.072
T63......	9.42	42.5	0.222	4.875	4.345	2.720	0.360	252	756	0.891	0.074
T61......	9.94	39.8	0.25	4.826	4.15	2.934	0.340	196	805	0.860	0.070
T60......	14.4	36.0	0.40	5.48	4.81	4.280	0.396	100.6	1,150	0.878	0.072
T59......	14.8	29.6	0.50	4.98	4.295	4.915	0.353	54.6	1,210	0.862	0.071
T58......	14.9	23.8	0.625	4.39	4.065	5.586	0.318	28.2	1,450	0.926	0.072
T54......	15.0	20.0	0.75	3.96	3.84	6.154	0.281	16.5	1,500	0.970	0.071
T57......	20.0	20.0	1.00	4.50	4.41	7.782	0.315	12.4	1,910	0.980	0.070
T56......	21.0	16.8	1.25	4.09	4.245	8.795	0.252	7.0	2,060	1.038	0.06
T55......	21.0	16.5	1.27	4.05	3.69	9.497	0.286	6.7	2,140	0.911	0.071

[a] Elevation of maximum depth of scour, in feet, near the end of the basin (Col. 12) and at the end of the wing wall (
caused bad erosion in back of wing wall; see duplicate run for normal conditions.

DATA, TURBINE ROOM SERIES T (b = 1 Ft)

$\frac{d'_2}{d_2}$ (10)	$\frac{c}{d_2}$ (11)	Scour, in Feet[a] Basin (12)	Wing Wall (13)	z_m (ft) (14)	$\frac{z_m}{d_2}$ [(15)	$\frac{d''_2}{d_2}$ (16)	Remarks (17)	Test No.
0.732	0.117	−0.21	−0.18	0.14	0.25	....	Conditions not as recommended[b]	T38
0.845	0.070	−0.03	−0.17	−0.01	−0.02	0.73	....	T39
0.874	0.118	−0.05	−0.08	0.11	0.22	0.79	Conditions not as recommended[b]	T37
0.835	0.066	−0.12	−0.48	0.32	0.28	0.63	....	T46
0.829	0.066	−0.01	−0.47	0.36	0.31	0.72	Air added to water, 1.24 cu ft per sec	T47
0.840	0.066	−0.11	−0.36	0.14	0.13	0.74	....	T44
0.860	0.069	−0.06	−0.27	0.17	0.17	0.69	Air added to water, 0.70 cu ft per sec	T45
0.838	0.069	−0.05	−0.24	0.18	0.20	0.82	....	T42
0.844	0.070	0.00	−0.19	0.15	0.16	....	Air added to water, 0.35 cu ft per sec	T43
0.878	0.075	0.00	−0.14	0.10	0.14	0.82	....	T40
0.823	0.057	−0.32	−0.37	0.20	0.12	....	....	T35
0.835	0.087	−0.23	−0.37	0.65	0.38	....	Conditions not as recommended[b]	T36
0.870	0.125	−0.14	−0.08	0.62	0.41	....	Conditions not as recommended[b]	T33
0.861	0.066	−0.19	−0.21	0.20	0.13	....	....	T34
0.841	0.138	−0.02	−0.19	0.43	0.31	....	Conditions not as recommended[b]	T29
0.854	0.073	−0.11	−0.21	0.11	0.08	....	....	T30
0.861	0.051	−0.14	−0.19	0.10	0.07	....	Conditions not as recommended[b]	T31
0.860	0.051	−0.06	−0.12	0.05	0.04	....	Air added to water, 0.40 cu ft per sec[b]	T32
0.889	0.245	0.25	−0.03	0.42	0.36	....	Conditions not as recommended[b]	T25
0.876	0.158	0.10	−0.02	0.35	0.29	....	Conditions not as recommended[b]	T26
0.881	0.082	0.02	−0.10	0.10	0.08	....	....	T27
0.890	0.051	−0.08	−0.02	0.07	0.06	....	Conditions not as recommended[b]	T28
1.032	0.294	0.14	0.24	0.24	0.24	....	Wing wall is too long[b]	T21
1.018	0.185	0.06	0.00	0.18	0.17	....	Conditions not as recommended[b]	T22
1.011	0.098	−0.13	0.00	0.12	0.12	....	Conditions not as recommended[b]	T23
1.019	0.061	−0.17	−0.03	0.17	0.17	....	....	T24
0.850	0.070	−0.10	0.26	0.29	0.10	0.50	Slope, top of wing wall, 1 on 1	T53
0.852	0.070	0.00	−0.31	0.33	0.14	0.64	....	T50
0.859	0.070	−0.02	0.27	0.36	0.15	....	Stepped wing wall	T51
0.859	0.070	0.00	0.23	0.28	0.12	....	Slope, top of wing wall, 1 on 1	T52
0.757	0.062	−0.23	−0.44	0.13	0.05	0.65	Conditions not as recommended[b]	T48
0.803	0.062	−0.24	−0.47	0.24	0.10	0.68	Air added to water, 0.46 cu ft per sec	T49
0.853	0.070	−0.05	−0.18	0.25	0.13	0.82	....	T41
0.847	0.0278	−0.45	−0.58	−0.34	−0.08	0.62	Conditions not as recommended[b]	T13
0.660	0.0477	−0.05	−0.21	0.07	0.02	....	Conditions not as recommended[b]	T14
0.747	0.0477	−0.09	−0.21	0.06	0.02	....	Conditions not as recommended[b]	T15
0.754	0.0758	0.18	0.23	0.68	0.17	....	Conditions not as recommended[b]	T16
0.853	0.0758	0.07	0.45	0.26	0.06	....	....	T17
0.710	0.0278	−0.50	−0.58	−0.19	−0.05	0.66	Conditions not as recommended[b]	T5
0.715	0.050	−0.23	−0.20	0.04	0.01	....	Conditions not as recommended[b]	T6
0.722	0.070	−0.10	−0.27	0.49	0.13	....	Conditions not as recommended[b]	T7
0.839	0.070	−0.09	−0.15	0.54	0.14	....	....	T8
0.884	0.0278	−0.51	−0.58	−0.33	−0.09	....	Conditions not as recommended[b]	T9
0.850	0.0295	−0.34	−0.28	−0.31	−0.09	0.66	Conditions not as recommended[b]	T1
0.780	0.050	−0.28	−0.23	−0.13	−0.04	....	Conditions not as recommended[b]	T2
0.789	0.070	−0.04	−0.10	0.45	0.13	....	Conditions not as recommended[b]	T3
0.795	0.100	0.18	....	0.84	0.25	....	Conditions not as recommended[b]	T4
0.852	0.051	−0.08	0.05	0.09	0.03	....	Conditions not as recommended[b]	T18
0.854	0.071	0.09	0.36	0.24	0.08	....	....	T19
0.862	0.102	0.02	0.30	0.48	0.17	....	Conditions not as recommended[b]	T20
1.008	0.0278	−0.08	0.15	0.45	0.20	0.89	Conditions not as recommended[b]	T10
0.997	0.112	0.14	0.30	0.376	0.17	....	Conditions not as recommended[b]	T11
1.000	0.224	0.27	0.35	0.50	0.22	....	Conditions not as recommended[b]	T12
0.865	0.072	−0.33	−0.02	0.61	0.12	0.55	Slope, top of wing wall, 1 on 1[c]	T62
0.891	0.074	−0.06	0.36	0.54	0.11	....	Slope, top of wing wall, 1 on 1	T63
0.860	0.070	0.03	0.50	0.57	0.12	0.57	Slope, top of wing wall, 1 on 1	T61
0.878	0.072	0.10	1.16	1.0	0.18	0.58	Slope, top of wing wall, 1 on 1	T60
0.862	0.071	0.05	1.04	0.67	0.13	0.68	Slope, top of wing wall, 1 on 1	T59
0.926	0.072	0.11	1.52	0.34	0.08	0.52	Slope, top of wing wall, 1 on 1	T58
0.970	0.071	−0.02	1.39	0.10	0.02	0.71	Slope, top of wing wall, 1 on 1	T54
0.980	0.070	0.04	1.78	0.89	0.20	....	Slope, top of wing wall, 1 on 1	T57
1.038	0.062	−0.12	1.90	0.84	0.21	....	Slope, top of wing wall, 1 on 1	T56
0.911	0.071	−0.02	2.21	0.41	0.10	....	Slope, top of wing wall, 1 on 1	T55

the wing wall (Col. 13). [b] Basin dimensions or controllable flow conditions not as recommended in this paper. [c] Washout

primarily to determine whether or not the end sill height equation originally proposed (Eq. 4) can be extrapolated beyond the range of the Reynolds numbers for which it was derived to those Reynolds numbers that are to be expected in full-size structures. These tests confirm the opinion that the extrapolated heights of end sill appear to be far too low. The results of the sixty-three tests made as part of the turbine room series are summarized in Table 6. The tests have been listed in order of increasing Reynolds numbers and grouped according to constant Froude numbers. The individual tests in each group have different amounts of air entrainment or different heights of end sill or different tailwater depths.

Typical scour patterns for several different heights of end sill are presented in Fig. 9. The height of end sill computed from Eq. 4 was used to obtain the results shown in Figs. 9(*a*), 9(*f*), and 9(*i*). The results of tests made with the end sill height recommended as a result of the turbine room series of tests are shown in Figs. 9(*b*), 9(*f*), and 9(*h*). The results presented in Figs. 9(*a*) to 9(*d*) and 9(*e*) to 9(*g*), inclusive, are for approximately the same Froude numbers (37 and 42, respectively), but at different Reynolds numbers; the tests in the latter group have a Reynolds number more than three and one-half times that of the first mentioned group (717,000 versus 200,000). The results shown in Figs. 9(*h*) and 9(*i*) are for a Froude number near the upper limit of what may be expected in a prototype structure (261) whereas the Reynolds number has a moderate value (710,000).

The 6-ft width of test channel was too narrow for many of the tests, with the result that certain features of the eroded sand bed are not indicative of what is to be expected in the prototype. These nontypical erosion patterns are shown in Figs. 9(*e*) to 9(*i*) near the downstream end and on the outside edge where a depression has been eroded along the side of the test channel. Typical erosion patterns are presented in Figs. 9(*a*) to 9(*d*). The tailwater depth for these latter tests was such that a stream bank could be maintained between the stream and the outside edge of the test channel. In the former tests the tailwater was so high that the stream bank could not be maintained and the disturbance caused by the water striking the vertical side of the channel resulted in the erosion of a depression along the channel wall.

Those tests of the turbine room series that have Reynolds numbers within the range covered by the tests of the culvert and flume outlet series check the previous conclusions. However, a study of the data obtained for Reynolds numbers beyond the range covered by the previous tests shows that the scour near the stilling basin is much less if an end sill height equal to 7% of the theoretical tailwater depth is used. Further study of the data shows that the height of the end sill relative to the tailwater depth (c/d_2) is independent of both the Froude and the Reynolds numbers. The data obtained during the flume and culvert outlet series were then re-analyzed and it was found that entirely satisfactory scour patterns were obtained for an end sill height equal to 7% of the theoretical tailwater depth. Admittedly, the end sill height as given by Eq. 4 resulted in slightly better scour patterns to $\mathbf{R} = 118{,}000$, where $c/d_2 = 0.07$, but the difference is so small as not to warrant the use of a separate

equation for the lower Reynolds numbers. As a result of all tests made to determine the height of end sill, the recommended equation is

$$\frac{c}{d_2} = 0.07 \qquad (5)$$

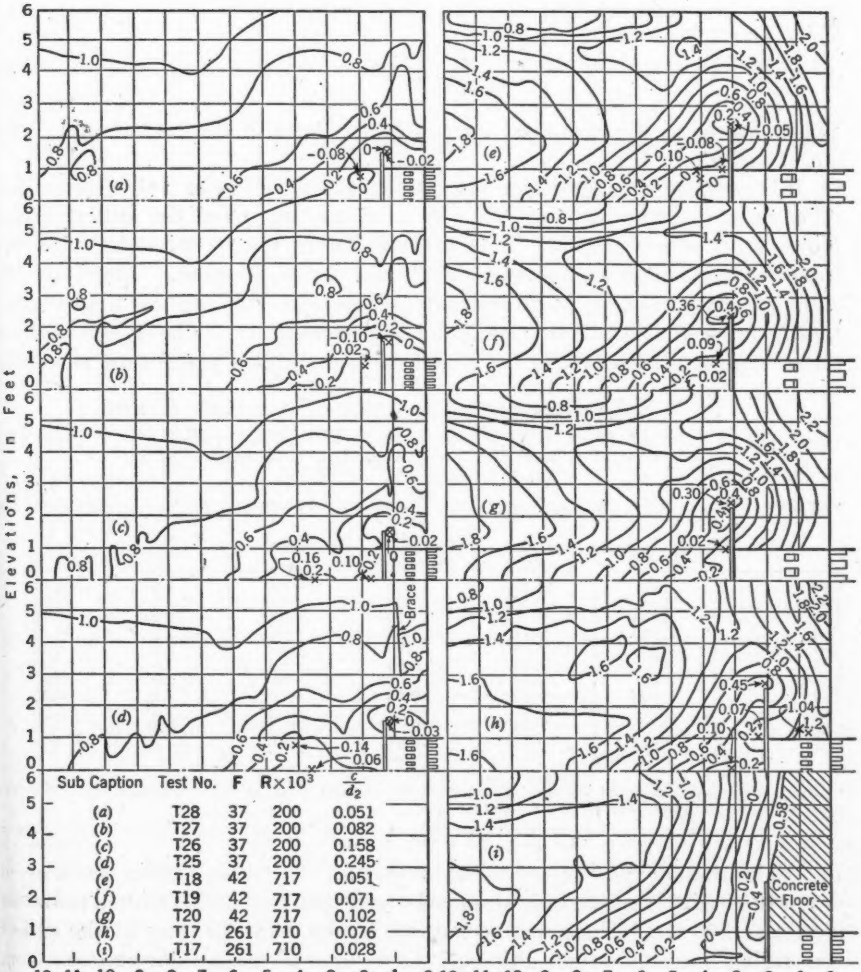


FIG. 9.—EFFECT OF END SILL HEIGHT, REYNOLDS NUMBER, AND FROUDE NUMBER ON SCOUR PATTERN

End sill heights as given by both Eqs. 4 and 5 were used for a few tests until it became certain that Eq. 4 would be eliminated from the final list of recommended design equations and Eq. 5 would be substituted for it. Subsequent tests were made simply to verify Eq. 5.

(*e*) *Tailwater Depth.*—A certain depth of tailwater above the stilling basin floor is required to prevent the hydraulic jump from being washed out of the stilling basin. This depth can ordinarily be computed from Eqs. 2. However, the use of the floor blocks and of the end sill makes it possible to reduce the actual tailwater depth d'_2 below the theoretical tailwater depth d_2 given by Eqs. 2. Mr. Warnock (4*b*) suggests a 15% reduction, or

$$\frac{d'_2}{d_2} = 0.85 \qquad (6a)$$

This tailwater depth was used in making the tests of the culvert and flume outlet series.

It is expected that the design tailwater elevation will be such that the surface roller on the hydraulic jump will never be washed out of the stilling basin. However, it is possible that the actual conditions will be somewhat different from those assumed at the time the structure was designed. Therefore, the stilling basin was tested at tailwater depths both greater and less than normal to determine what would happen under these conditions. The results of these tests are presented in Table 7. Water-surface and bed profiles are shown in

TABLE 7.—EFFECT OF ABNORMAL TAILWATER DEPTH
(L_r = 12; b_1 = 9.0 Ft; d_1 = 1.0 Ft; d_2 (Average) = 7.20 Ft; L_B = 10.92 Ft; c = 1.00 Ft; F (Average) = 29.5; L_B/d_2 = 1.52; c/d_2 = 0.14; Stilling Basin Floor Elevation = − 5.00 Ft)

Test No.	Flow, Q (ft³/sec)	d'_2 (ft)	$\frac{d'_2}{d_2}$	Scour[a]	z_a (ft)	$\frac{z_a}{d_2}$
C92	277	11.49	1.60	−4.0	1.8	0.11
C91	274	9.56	1.33	−4.0	0.8	0.11
C90	277	8.54	1.19	−4.0	1.1	0.15
C83	286	7.71	1.07	−4.0	1.2	0.17
C84	280	7.08	0.98	−4.0	1.0	0.14
C85	272	6.03	0.84	−4.1	0.8	0.11
C86	280	5.00	0.69	−5.0	7.0[b]	0.97
C87	280	3.80	0.53	−7.0	7.1[b]	0.99
C88	277	2.77	0.38	−8.5	7.4[b]	1.03
C89	275	1.80	0.25	−10.1	7.9[b]	1.10
C98	284	−0.40	−0.06	−13.0	10.5[b]	1.46

[a] Elevation of maximum depth of scour, in feet, on channel center line near end of stilling basin. [b] Surface roller on hydraulic jump washed out of stilling basin.

Fig. 10 for normal and lowered tailwater levels. The surface roller was washed out of the basin when the tailwater level was reduced to 5.00 ft (normal tailwater level was 6.00 ft). There was no increase in the depth of scour at the end of the stilling basin, however, although the volume of erosion downstream from the basin was greatly increased. Even if no cutoff wall is used, the stability of the structure is not endangered for this tailwater level, although the stilling basin action is impaired. If the tailwater depth falls below this level, however, it is absolutely necessary that a cutoff wall be used.

The following brief explanation will define the term "air-jet" boundaries, in Fig. 10. The turbulence at the entrance to the stilling basin causes much air to be entrained at that point. In addition, the water in the channel approaching the stilling basin may contain entrained air. The vigorous turbu-

lence in and beyond the stilling basin prevents the ready separation of this entrained air. Therefore, the presence of air in the water may be taken as an indication that the flow is very turbulent and, conversely, the absence of air may be assumed to indicate that the turbulence is insufficient to maintain the air in the "suspended" state. The lower boundary of the "air jet" shown in Fig. 10 is the lower limit of air entrainment and therefore indicates the lower limit of extreme turbulence as defined. The upper boundary of the "air jet" is taken as the lower boundary of the roller normally associated with the hydraulic jump. Much entrained air is also found in this roller.

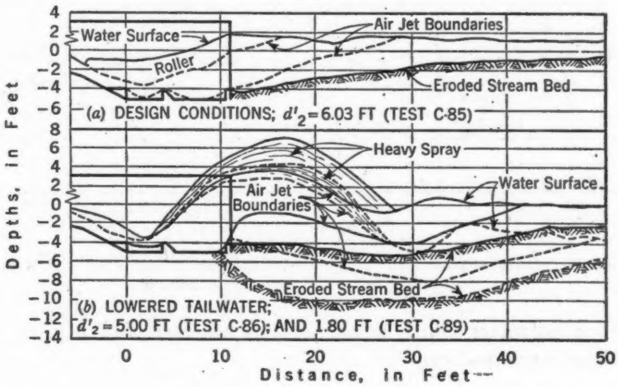


FIG. 10.—CENTER-LINE BED AND WATER-SURFACE PROFILES FOR NORMAL AND SUBNORMAL TAILWATER DEPTHS

Unfortunately, in making a check of the stilling basin design during the flume outlet series, it was found that the roller ordinarily present on the hydraulic jump was washed out of the stilling basin at values of the Froude number of less than about $\mathbf{F} = 30$. Several methods were available to overcome this difficulty: The tailwater depth could be increased, the end sill height could be increased, or the stilling basin could be lengthened. Increasing the end sill height increased the height of the boil caused by the end sill and increased the depth of scour near the end of the stilling basin. The increase required in the length of the stilling basin to insure its proper functioning was so great as to be uneconomical. The method finally adopted was to increase the tailwater depth. The equation for that part of the tailwater depth curve between $\mathbf{F} = 3$ and $\mathbf{F} = 30$ was therefore changed to

$$\frac{d'_2}{d_2} = 1.10 - \frac{\mathbf{F}}{120} \quad \text{.....................(6b)}$$

to insure the proper functioning of the stilling basin at the lower Froude numbers. This curve and the data on which it is based are shown in Fig. 11.

For many runs of the turbine room series the tailwater depth was slowly lowered until the roller was washed out of the stilling basin. The object of this procedure was to determine the relative tailwater depth at which the basin would cease to function as was assumed in the design and to determine the

safety factor for this part of the stilling basin. The results of these tests are presented in Table 6 and Fig. 11. They indicate that d'_2 can be reduced below $0.85\, d_2$ when the Froude number is high—that is, when $\mathbf{F} > 120$. A curve based on the turbine room tests has been added to Fig. 11 to permit a lowering

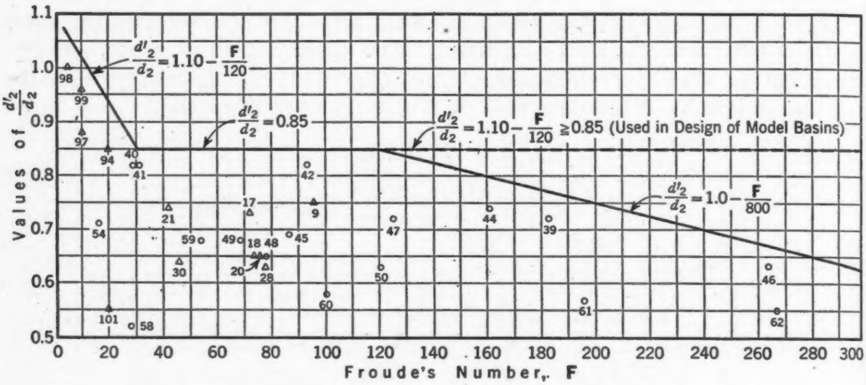


FIG. 11.—DIMENSIONLESS PLOT SHOWING TAILWATER DEPTH AS A FUNCTION OF THE FROUDE NUMBER (SEE EQS. 6)

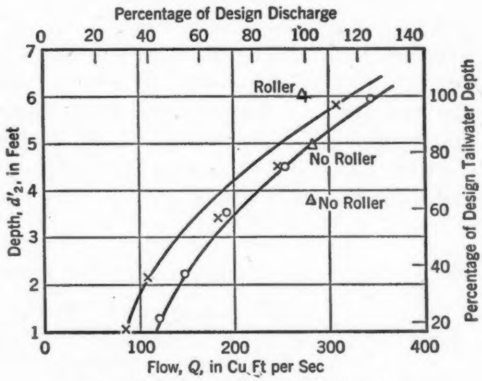
of the relative tailwater depth below $0.85\, d_2$ for values of $\mathbf{F}$ greater than 120. This curve has the equation,

$$\frac{d'_2}{d_2} = 1.00 - \frac{\mathbf{F}}{800} \qquad (6c)$$

The difference between the curve and the plotted points of Fig. 11 is the safety factor which insures that the stilling basin will perform as anticipated by the designer. The safety factor is small, but it is felt to be sufficient since a tailwater level somewhat lower than the design value will not endanger the basin, although the roller will be washed out of it. For this condition the floor blocks and end sill deflect the stream into the air and break it up as is shown by Fig. 10(*b*) where the tailwater depth is reduced to 5 ft (the design depth being 6 ft). This statement is inserted not as a justification for using a tailwater depth below that recommended but to show that an unanticipated lowering of the design tailwater depth or a discharge greater than that for which the outlet is designed will not endanger the outlet structure.

During the culvert outlet series a few tests were made in which the tailwater level was held at a constant elevation while the discharge was increased until the roller was washed out of the stilling basin. These tests were made to determine what would happen if the tailwater level were lower than was assumed by the designer or the actual discharge were greater than that assumed for design purposes. Conversely, the discharge was increased for several different tailwater elevations to wash the roller out of the stilling basin and then was decreased until the roller reappeared. The results of these tests indicate the maximum discharge at which the basin will continue to function properly for any tailwater depth. It is apparent from Fig. 12 that the stilling basin on

which these tests were made would function properly for the design tailwater depth until a discharge equal to about 118% of the design value was reached. Similarly, for the design discharge, the basin would function properly until the tailwater depth was reduced to 90% of the design value or 76% of the theoretical value. When the tailwater depth was increased to 7.20 ft or 120% of the design (102% of the theoretical) value, the roller could not be washed out of the stilling basin even though the discharge was increased to 598 cu ft per sec or 217% of the design discharge.



o Point at Which Roller is Washed Out of Stilling Basin for Increasing Flow

× Point at Which Roller Appears in Stilling Basin for Decreasing Flow

Δ Conditions Observed During Test of Stilling Basin

\+ Design Point

FIG. 12.—EFFECT OF VARIATIONS IN DISCHARGE AND TAILWATER DEPTH ON STILLING BASIN PERFORMANCE

(*f*) *Side-Wall Height.*—The flow in the stilling basin is very turbulent and the water surface is rough so that some freeboard above tailwater level is necessary if overtopping of the side walls is to be prevented. In addition to the surface roughness, the standing wave or boil, caused by the floor blocks and end sill, requires freeboard above the tailwater level. For Froude numbers less than about **F** = 20 the crest of the standing wave is in the stilling basin whereas for higher Froude numbers the crest occurs downstream from the end of the basin and its full height need not be considered in designing the side-wall height.

Average profiles of the water surface in the stilling basin were obtained for all series, but the maximum height of splash was obtained only for the turbine room series. From the latter tests the height of the side wall is determined. The maximum height of the splash z_m in the stilling basin was divided by d_2. Both z_m and z_m/d_2 are shown in Table 6. Although there is considerable scatter to the data, z_m/d_2 is apparently independent of **F**. The maximum value of z_m/d_2 for those tests of the turbine room series in which the recommended design conditions exist is 0.31; and the minimum, − 0.02. The maximum value of z_m/d_2 occurs at the exit end of the stilling basin. A study of the data shows that, if the height of the stilling basin side walls z above the maximum tailwater level is given by the equation—

$$z = \frac{d_2}{3} \quad \ldots\ldots\ldots\ldots\ldots\ldots (7)$$

—the freeboard will be sufficient to keep the splash in the stilling basin. Because of the scatter in the data, the freeboard provided by this equation will be greater, in some cases, than is necessary to protect the structure adequately,

but the safety factor is not excessive for the average case. Two values of z_m/d_2 were obtained from motion pictures made during the culvert and flume series of tests (series C and F). These observations substantiate the results of the turbine room tests (series T).

Frequently the elevations of the tailwater and stream bed at the time the stilling basin is constructed are much higher than those that are ultimately expected and for which the stilling basin is designed. In this case the height of the side wall must be based on the maximum expected tailwater level. If the side walls are constructed so low that water will pour over them into the stilling basin, the basin must handle, in effect, a quantity of water greater than that for which it was designed. The result is the large increase in the size of the scour hole shown in Fig. 13. This condition occurs because the 'air jet,"

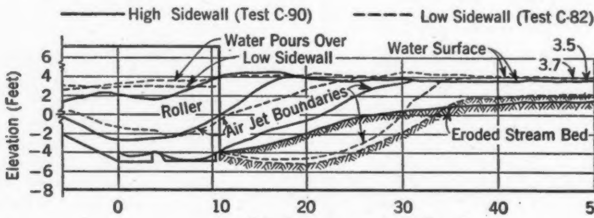
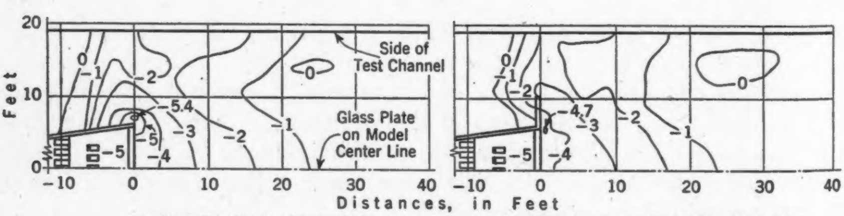


FIG. 13.—EFFECT OF WATER POURING OVER LOW SIDE WALLS INTO STILLING BASIN ON CENTER-LINE BED AND WATER-SURFACE PROFILES

leaving the stilling basin is not deflected upward by the end sill as much as if no water entered the basin over the top of the side walls. Consequently, the side walls should always be high enough to extend above the maximum tailwater level.

(*g*) *Wing Walls.*—The SAF stilling basin was developed mainly for use in connection with earth dams. For this type of dam, wing walls are used at the end of the stilling basin to protect the toe.

Shape of Wing Walls.—The wing walls used in the past have been rectangular in downstream elevation. During the culvert outlet series (C) tests



(*a*) NO WING WALL (TEST C-46) (*b*) WING WALL 4.5 FT. LONG (TEST C-47)

FIG. 14.—EFFECT OF RECTANGULAR WING WALL ON SCOUR PATTERN

were made to determine the best length of this wing wall. During runs made with stilling basins having no wing walls it was observed that there was considerable scour at the toe of the dam and that the deepest scour occurred close to the end of the stilling basin side walls, as shown in Fig. 14(*a*). The principal

cause of the scour is an eddy along the side of the channel which is set in motion and is driven by the water that flows along the center of the downstream channel after it leaves the stilling basin. The upstream end of this eddy causes the scour at the toe of the dam. The wing wall has a twofold function: It calms the water in back of it and prevents the eddy from extending as far upstream as it would if the wing wall were absent; and it lessens the concentration of flow at the end of the stilling basin. The use of a wing wall results in a decreased depth of scour at the end of the stilling basin (Fig. 14(*b*)), although it has little or no effect on the scour at the channel center line.

The results of the tests made to determine the best length of rectangular wing wall are presented in Table 8. Too short wing walls are ineffective in

TABLE 8.—EFFECT OF WING-WALL LENGTH ON DEPTH OF SCOUR; SERIES C

($L_r = 12$; $b_1 = 9$ Ft; $Q = 275$ Cu Ft per Sec; $d_1 = 1$ Ft; $d_2 = 7.13$ Ft; $d'_2 = 6$ Ft (Which Equals El. + 1.0); $L_B = 8.75$ Ft; $c = 1$ Ft; $F = 29$; $d'_2/d_2 = 0.84$; $L_B/d_2 = 1.23$; $c/d_2 = 0.14$; and Stilling Basin Floor Elevation = − 5.0 Ft)

Test No.	Length[a] (%)	Elevation of Maximum Depth of Scour, in Feet		
		Maximum	Place where maximum occurred	Channel[b]
C46	0	−5.4	In line with the end of the stilling basin and 2.5 ft outside the side wall	−4.2
C48	32	−5.0	At end of wing wall	−4.3
C54	42	−4.5	In line with the stilling basin side wall and close to the end of the basin	−4.0
C47	47	−4.7		−4.0
C49	53	−4.5		−4.2

[a] Length of the wing wall expressed as a percentage of the side-wall height. [b] Depth of scour on the channel center line, in feet, near the end of the stilling basin.

preventing scour and there is nothing to be gained by making the wing walls too long. As a result of these tests, it was suggested that the minimum length of wing wall be 40% of the stilling basin side-wall height. Lengths longer than this do not change the hydraulic performance significantly.

This rectangular wing wall was not entirely satisfactory since the depth of scour at its end was sufficient to cause some concern. It was felt that the concentration of flow lines at the end of the wing wall caused this scour. A submerged extension of the wing wall to keep the concentrated streamlines above the stream bed was tried. The height of this extension above the stilling basin floor was 0.5 d'_2 and the length was 0.6 times the side-wall height. The results were excellent in that the depth of scour at the end of the wing wall and (what is just as important) the erosion in back of the wing wall were considerably decreased. The results of this test are shown in Fig. 15(*b*). The results of a comparable test made with the originally proposed wing-wall length are presented in Fig. 15(*a*) for purposes of comparison. Subsequent to this test an additional test was made in which the top of the wing wall was sloped. The slope used was 1 on 1 beginning at the top of the stilling basin side wall. The results of this test (see Fig. 15(*c*)) were such that this wing-wall shape was used for all subsequent tests and has been shown in Fig. 1 as the recommended wing-wall shape.

Location of Wing Walls.—In the past, the wing walls used on most outlets built by the SCS have been placed perpendicular to the center line of the outlet structure. As a result, this arrangement was used throughout the test program. However, after the SAF stilling basin design had been released, ques-

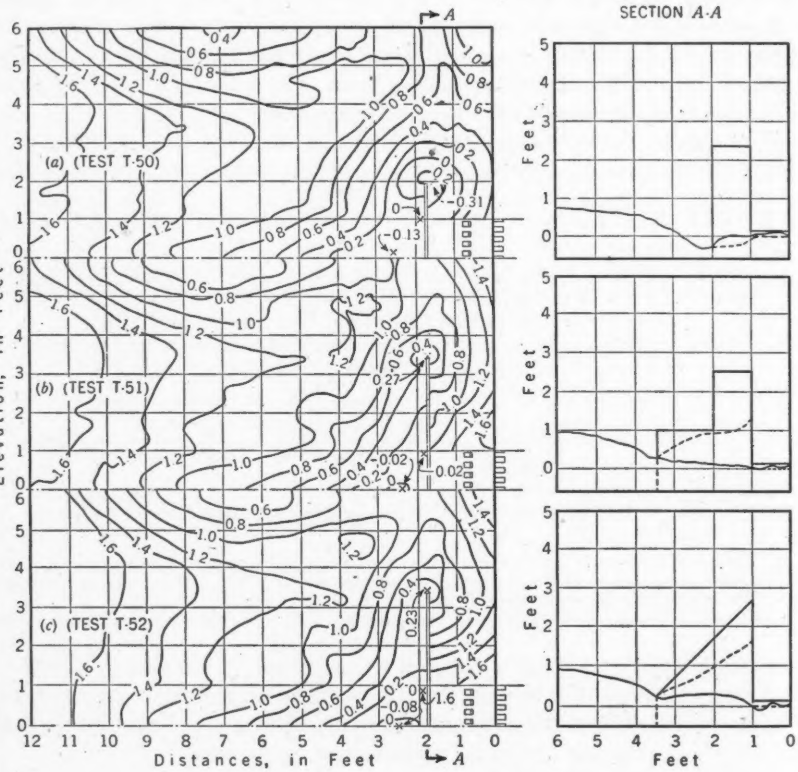
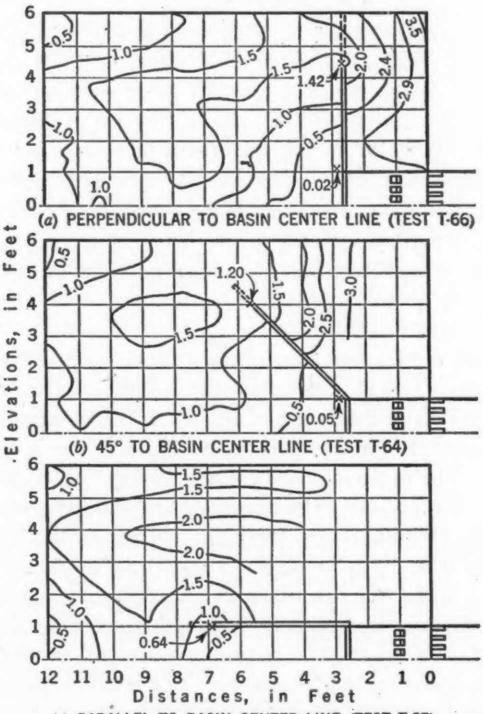


FIG. 15.—EFFECT OF WING-WALL SLOPE ON SCOUR PATTERN

tions were raised as to whether the wing wall should be perpendicular, parallel, or at some intermediate angle to the basin center line. Tests were made to answer these questions.

The stilling basin used in this group of tests had the dimensions shown in Table 6 for test T63. The stilling basin model had deteriorated somewhat in the fifteen months since test T63 was made, and the turbine room test setup had been partly dismantled. As a result, no attempt was made to measure the discharge, although the same valve setting was used for each of the tests in this series. It is believed that the flow was somewhat greater than that for test T63. Less sand was also available for the movable bed than was desirable. Therefore, the test results are only comparable among themselves. Plan views of each of the stilling basins and wing-wall arrangements tested are shown in Fig. 16. The top of the wing wall was sloped at 1 on 1 in all cases.

The results of the tests made on three different positions of the wing wall are given in Fig. 16. The boil height for test T66 was satisfactory ($z_m/d_2 = 0.10$) and the scour pattern (Fig. 16(a)) was similar to that for other tests made with the wing wall placed perpendicular to the basin center line. The maximum boil height for test T64 was 0.6 ft greater at the end of the stilling basin ($z_m/d_2 = 0.23$) than for test T66, but was still below the top of the basin side wall. The scour pattern was changed somewhat (Fig. 16(b)) and in general



(c) PARALLEL TO BASIN CENTER LINE (TEST T-65)

FIG. 16.—EFFECT OF TRIANGULAR WING-WALL POSITION ON SCOUR PATTERN

was improved. Erosion of the dam fill beside the stilling basin was decreased by a foot or more. The boil height for test T65 was the highest observed in this group of tests ($z_m/d_2 = 0.34$). This boil height occurred at the very end of the stilling basin. The high boil did, however, cause a deeper secondary scour hole than for the other two tests of this group. The scour pattern (Fig. 16(c)) for this test was also satisfactory. In no tests of this group did the scour descend to the elevation of the stilling basin floor, although the rise in the bed elevation along the channel center line downstream from the basin was less as the wing wall was brought nearer the outlet center line. For tests T64 and T65 there was an opening between the stilling basin wall and the wing wall that was not noticed until the tests had been completed. Water passed through this opening and caused an erroneous scour pattern at this point on the outside of the

basin wall. Contours believed to be erroneous have been omitted from Figs. 16(*b*) and 16(*c*).

These tests indicate that the wing wall may be extended parallel to the basin center line if field conditions make it necessary to do so although the boil height is considerably increased. The wing wall perpendicular to the stilling basin center line produces entirely satisfactory flow and scour conditions, but the best over-all conditions were obtained when the wing wall was set at an angle of 45° to the outlet center line.

(*h*) *Shape of Stilling Basin.*—The size of the stilling basin varies with the initial flow depth. A reduction in d_1 will decrease d_2, the length of the basin, the height of the side walls, and the depth of excavation necessary to secure the required tailwater depth. In addition, a larger percentage of the energy in the water as it enters the stilling basin will be dissipated. A saving in the over-all cost of the outlet will ordinarily be possible if a transition is placed between the culvert or flume and the stilling basin to accomplish this reduction in d_1. In cases where a transition is used, a diverging transition side wall can be extended to form the stilling basin walls, as is shown on a half plan of Fig. 1. The limiting basin side-wall divergence is unknown but is probably as large as can be used on the transition.

A few tests were made on a trapezoidal-shaped stilling basin as part of the culvert outlet series. The stilling basin was designed for flow conditions at its entrance and the width and spacing of the floor blocks were multiplied by the ratio b_2/b_1 to compensate for the increase in width of the stilling basin at their location. (The widths b_1 and b_2 are defined in Fig. 1.) All blocks had their axes parallel to the center line of the basin. Flow conditions in the downstream channel were somewhat improved through the use of the trapezoidal stilling basin, as a result of the lower velocity of exit from the basin and because the stream spreads out to fill the downstream channel and reduces the size of the eddies that are formed along the sides of the channel near the stilling basin.

(*i*) *Cutoff Wall.*—A cutoff wall is used at the end of the stilling basin to prevent scour from undermining the basin. The depth of the cutoff wall, therefore, must be greater than the maximum depth of scour at the end of the stilling basin.

Serious erosion near the end of the stilling basin is prevented by the end sill which deflects the jet, leaving the basin upward. A ground roller under the jet brings material upstream and further aids in preventing erosion. During the tests of the recommended design the scour sometimes reached an elevation slightly below the floor of the stilling basin but never reached a depth at the end of the basin greater than the thickness of a floor slab that might be used. Therefore, a cutoff wall of only nominal depth need be used at the end of the stilling basin.

(*j*) *Effect of Air Entrainment.*—Air is ordinarily entrained by the water flowing in channels laid on a steep slope—resulting in a greatly increased depth of flow of the mixture. However, no air was naturally entrained during the model tests because of the low velocities or short length of test channel. Therefore, five runs were made in which from 10% to 117% of air was mixed with

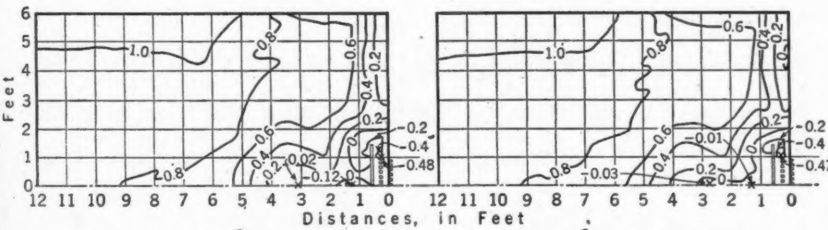
the water. The results of these tests and their parallel runs made without air entrainment are summarized in Table 6.

It was intended that the quantity of air mixed with water as it passed under the depth control gate (see Fig. 3) be computed from the equation:

$$Q_a = 0.2\, Q \sqrt{\mathbf{F}_{aw}} - Q \ldots\ldots\ldots\ldots\ldots\ldots\ldots (8)$$

in which Q_a is the air flow at atmospheric pressure and Q is the water flow, both in cubic feet per second, and $\mathbf{F}_{aw}$ is the Froude number for the air-water mixture. Eq. 8 is an average of the equations derived by Warren DeLapp, Jun. ASCE (11*a*), from an analysis of the data published by L. Standish Hall, M. ASCE (11). However, an error was made in the solution of the equation and the actual quantities of air used are somewhat different from those intended. Nevertheless, the qualitative results give an indication of what may be expected when air is entrained with the water.

In designing the test basins, it was assumed that the velocity of the water would be unchanged by entrained air. The initial depth of flow was increased, therefore, to compensate for the larger total quantity of mixture flowing in order to keep the velocity constant. The result is a lowering of the Froude



(*a*) F = 264; R = 99000; $\frac{Q_a}{Q}$ = 0.00 (TEST T-46) (*b*) F = 125; $\frac{Q_a}{Q}$ = 1.17 (TEST T-47)

FIG. 17.—EFFECT OF ENTRAINED AIR ON SCOUR PATTERN

number for the flow of mixture below that obtained for water flow alone. The stilling basin was designed for the higher Froude number obtained when there was no air entrainment and the same basin was used for the test with air entrainment. In either case the tailwater depth is the same whether or not air entrainment occurs since the downstream velocities are low enough that the air separates from the water after leaving the stilling basin. No increase in side-wall height is necessary when the water transports entrained air. The results of a typical test are given in Fig. 17(*b*) where the percentage of air used was 117—that is, the air flow was greater than the water flow. The results presented in Fig. 17(*a*) were obtained on the same stilling basin when no air was entrained in the water.

These tests show that the effect of air entrainment can be neglected in the design of the SAF stilling basin. The resulting structure will safely handle any flow in which air is entrained.

7. Summary

The following conclusions are reached as a result of the tests made to develop and verify the SAF stilling basin design.

1. The length of the stilling basin for Froude numbers between **F** equals 3 and **F** equals 300 is determined by Eq. 3 $\left(\frac{L_B}{d_2} = \frac{4.5}{\mathbf{F}^{0.38}}\right)$.

2. The height of the chute blocks and floor blocks is d_1 and the width and spacing are approximately $\frac{3}{4}\,d_1$.

3. The distance from the upstream end of the stilling basin to the floor blocks is $\frac{L_B}{3}$.

4. No floor block should be placed closer to the side wall than $\frac{3}{8}\,d_1$.

5. The floor blocks should be placed downstream from the openings between the chute blocks.

6. The floor blocks should occupy between 40% and 55% of the stilling basin width.

7. The widths and spacings of the floor blocks for diverging stilling basins should be increased in proportion to the increase in stilling basin width at the floor block location.

8. The height of end sill is given by Eq. 5 $\left(\frac{c}{d_2} = 0.07\right)$.

9. The depth of tailwater above the stilling basin floor is given by Eq. 6*b* $\left(\frac{d'_2}{d_2} = 1.10 - \frac{\mathbf{F}}{120}\right)$, for **F** equals from 3 to 30; by Eq. 6*a* $\left(\frac{d'_2}{d_2} = 0.85\right)$, for **F** equals from 30 to 120; and, by Eq. 6*c* $\left(\frac{d'_2}{d_2} = 1.00 - \frac{\mathbf{F}}{800}\right)$, for **F** equals from 120 to 300.

10. The height of the side wall above the maximum tailwater depth to be expected during the life of the structure is given by Eq. 7 $\left(z = \frac{d_2}{3}\right)$.

11. Wing walls should be equal in height to the stilling basin side walls. The top of the wing wall should have a slope of 1 on 1.

12. The wing wall should be placed at an angle of 45° to the outlet center line.

13. The stilling basin side walls may be parallel (rectangular stilling basin) or they may diverge as an extension of the transition side walls (trapezoidal stilling basin).

14. A cutoff wall of nominal depth should be used at the end of the stilling basin.

15. The effect of entrained air should be neglected in the design of the stilling basin.

8. Remarks

The safety factor incorporated in the design of the SAF stilling basin is low but it is felt to be adequate. As more exact methods of analysis are applied to the design of a structure, it is possible to reduce the safety factor, with increased assurance that the structure will perform as intended by the designer. The SAF stilling basin is based on a large number of tests and knowledge of its performance is such that the safety factor can be safely reduced to a minimum. In addition, the tests show that, although a discharge somewhat higher than the design discharge might wash the roller out of the basin, this action would do little or no damage. A lowering of the tailwater somewhat below the design level would have the same effect and result.

Throughout the tests it was observed that the performance of the SAF stilling basin was excellent at discharges less than the design value. For the design conditions, the SAF stilling basin provides an economical method of dissipating energy and preventing dangerous and uncontrolled stream bed erosion. The hydraulic design of the SAF stilling basin is simple; only the depth and velocity of the flow approaching the basin are needed before entering a design chart (12) or solving the design equations.

9. Acknowledgments

The work covered in this paper was performed at the St. Anthony Falls Hydraulic Laboratory of the University of Minnesota where the Office of Research of the SCS and the Minnesota Agricultural Experiment Station are cooperating in the solution of problems pertinent to the activities of the SCS. The work was done under the administrative direction of M. L. Nichols, chief, Office of Research, and under the technical direction of C. E. Ramser, M. ASCE, research specialist in hydrology. The tests were largely performed by Charles A. Donnelly, senior engineering aide, under the direction of the writer. Albert N. Huff was project supervisor at Minneapolis during these investigations. From him and from Donald A. Parsons much constructive criticism was forthcoming. The writer also wishes to acknowledge the most helpful, constructive criticism offered by the anonymous (to the writer), expert reviewers to whom the paper was submitted by the Society's Committee on Publications.

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APPENDIX II.—NOTATION

The following letter symbols, adopted for use in this paper and for the guidance of discussers, conform essentially to American Standard Letter Symbols for Hydraulics (ASA—Z10.2—1942), prepared by a Committee of the American Standards Association, with Society representation and approved by the Association in 1942. In addition, the symbols of linear concepts are defined by Fig. 1.

b = stilling basin width:
- b_1 = at the entrance;
- b_2 = at the floor block location;
- b_3 = at the exit;

c = height of end sill;

d = flow depth:
- d_1 = at the stilling basin entrance (lower conjugate depth for the hydraulic jump);
- d_2 = upper conjugate depth for the hydraulic jump (see Eq. 2*b*);
- d'_2 = tailwater depth, measured above the stilling basin floor;
- d''_2 = tailwater depth at which the hydraulic jump was washed out of the stilling basin;

$\mathbf{F}$ = the Froude number (Eq. 1*a*); $\mathbf{F}_{aw}$ = the Froude number when air is entrained;

g = acceleration due to the effect of gravity;

L_B = length of the stilling basin;

L_r = assumed linear scale ratio between prototype and model;

Q = rate of flow of water; Q_a = rate of flow of air;

$\mathbf{R}$ = the Reynolds number (Eq. 1*b*); $\mathbf{R}_m$ = the Reynolds number for the model;

V_1 = velocity of flow at stilling basin entrance;

z = height of stilling basin side walls above maximum tailwater level:
- z_a = average maximum boil height above tailwater level;
- z_m = maximum boil height in stilling basin above tailwater level; and

ν = kinematic viscosity.

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AMERICAN SOCIETY OF CIVIL ENGINEERS

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PAPERS

THE MARINE OPERATING PROBLEMS, PANAMA CANAL, AND THE SOLUTION

By Miles P. DuVal,[1] Esq.

Synopsis

Reconstruction of the Panama Canal to facilitate its operation and to increase its capacity is imperative. The present design is a high level waterway, 85 ft above sea level. An improvement near the Pacific end of the Canal to provide a high level terminal lake analogous to Gatun Lake on the Atlantic side is discussed in this paper.

The need for the change is explained, based on marine operating and safety considerations. The solution proposed is the physical removal of Pedro Miguel Locks, the construction of all Pacific locks in continuous lifts near Miraflores, the elevation of the Miraflores dams, and the creation of a high level anchorage north of Miraflores. Navigational rather than engineering features are stressed.

This paper was originally presented before a meeting of the Panama Section of the Society on May 20, 1943. The opinions and recommendations are purely personal, and have no official connotation.

Present Canal Operating Plan

Any mariner's first transit of the Panama Canal is always a memorable experience. Although he may know only a little of the history of this great waterway he is always deeply impressed by the magnitude of the Canal and the quiet efficiency of its operation.

His ship passes through the Atlantic (Caribbean) sea level section (Fig. 1) and enters the three-lift Gatun Locks. The ship is locked up to the summit level in one continuous operation. After traversing Gatun Lake, it enters the tortuous and rocky artificial gorge known as Gaillard (formerly Culebra) Cut. At the south end of this cut the vessel enters the single-lift Pedro Miguel Locks and is locked down to the intermediate-level Miraflores Lake. It then enters a

Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by July 1, 1947.

[1] Capt., U. S. Navy Dept., Washington, D. C.; formerly Capt. of Port, Balboa, Canal Zone, March, 1941, to June, 1944.

second set of locks at Miraflores and is locked down in two steps to the Pacific sea level.

Many shipmasters observe the Canal and the lock arrangements closely. Instinctively they note that only one lock structure was placed on the Atlantic side, and they wonder why two sets of locks are needed on the Pacific side in what they feel is an improper marine arrangement. They would prefer one

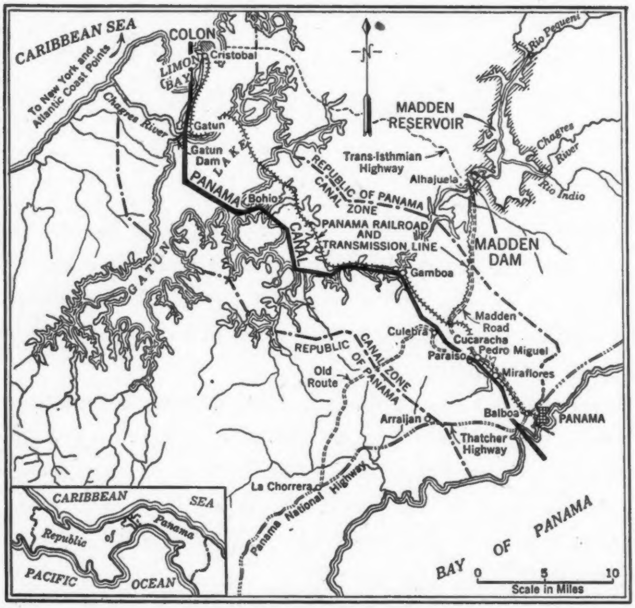

Fig. 1.—General Map of the Panama Canal Area

structure on the Pacific as at Gatun to avoid (1) slowing their vessels in a rocky gorge and (2) undertaking double handling.

Masters generally do not know the long history of the Canal nor the great struggles involved in its building; so they are reminded that this idea of single-lock structures at the Pacific terminus is not new and that it was considered during construction but had to be discarded because it was claimed that suitable foundations were not available and because no other solution seemed possible. This explanation is accepted; and a transit is usually sufficient to impress masters with some of the marine problems affecting operation of the Canal.

The principal marine problems are the traffic bottleneck at Pedro Miguel, lockage surges in Gaillard Cut, fogs, currents, and the suction of banks or lock walls. Only after long service with the Canal and close observation do shipmasters comprehend fully the nature of the operational difficulties and hazards of this waterway.

In addition to the inherent navigational problems of a restricted waterway, the physical layout of the Canal for operation is not symmetrical. On the Atlantic side there is a capacious summit level anchorage in Gatun Lake where

vessels bound in either direction can anchor safely until ready to proceed. Such favorable facilities do not exist on the Pacific side. The Pedro Miguel Locks are located squarely at the south end of Gaillard Cut, with no summit level anchorage available as in Gatun Lake. These locks serve as a timing device that restricts the use of the cut to the capacity of the locks, and thus limit the capacity of the Canal.

In spite of these inherent problems, the Canal has operated successfully but it has operated under difficulties. Northbound vessels enter the cut at lockage intervals and no faster. Southbound vessels cannot arrive more rapidly than the locks can be readied to receive them. Hence, the Canal has not been able to develop its maximum obtainable capacity.

This condition can be illustrated by examining a day's traffic in the Canal under the restricted conditions. Vessels start transits on both sides of the Isthmus simultaneously, arriving at Gatun Locks and Miraflores Locks about 7:00 a.m. Northbound vessels are locked up to the summit level continuously until all northbound traffic has passed Pedro Miguel Locks. Then each proceeds in succession at lockage intervals through the cut into Gatun Lake and thus to Gatun where it enters the locks or anchors to await down lockage. Southbound vessels are locked up to Gatun Lake where they anchor at Gatun anchorage or wait under way until they are scheduled to enter the cut. After the northbound traffic clears Gaillard Cut at Gamboa, the southbound vessels spaced at lockage intervals enter the cut.

Thus, Gatun anchorage supplies a stopover station for both northbound and southbound vessels and permits flexible operation of Gatun Locks. At Pedro Miguel there is no comparable anchorage to enter. Vessels have to approach these locks in a relatively narrow and rocky passage; they cannot anchor for they would swing into the bank; they cannot slow too much because of the necessity of maintaining steerage way in a narrow rocky gorge which in the dry season is subject to high winds in the daytime. Thus, vessels must be received as they arrive and they cannot arrive faster than they can be received. This situation creates immense ship handling and traffic control problems that cause delays and at times subject both the Canal and the transiting vessels to danger.

For marine operations the location of the Pedro Miguel Locks at the end of Gaillard Cut constitutes the great traffic bottleneck of the Panama Canal. It was the fundamental error in the design of the Canal.

Canal Capacity

What is the capacity of the Panama Canal? Obviously, if the summit water supply is ample, it is the capacity of the locks. Because of the bottleneck at Pedro Miguel Locks, their capacity measures that of the Canal.

Assuming that one side of the Pedro Miguel Locks is in operation during biennial overhauls and that the lockage interval for single culvert operations is $53\frac{1}{2}$ min, the 24-hour capacity is twenty-seven lockages. This figure has been accepted as the minimum capacity of the Panama Canal.

A more complete picture of canal capacity was presented in a study by the Locks Division of the Panama Canal in 1938 (Table 1). Two methods of operating the Gatun Locks are noted. In the one designated "normal" a

vessel enters a lock after the vessel ahead has cleared the distant chamber; in the "follow-up" operation a vessel enters a lock chamber before the preceding vessel has cleared the last chamber. Thus the vessels are always separated by one lock. It should also be mentioned that the filling may be accomplished by the use of either single or double culverts as shown in Table 1, for each lock has two sets of culverts.

TABLE 1.—CAPACITY OF SINGLE LOCKS, PANAMA CANAL; BASED ON STUDY BY LOCKS DIVISION, 1938

Lock	Normal Intervals (Min)		Total 24-Hr Capacity	
	Single culvert	Double culvert	Single culvert	Double culvert
Gatun:				
Follow-up	54	40	26	35
Normal	91	76	16	19
Pedro Miguel	53.5	37.5	27	38
Miraflores	60	48.5	24	29

Thus, the lockage capacity of the Panama Canal is lowest during the overhaul of Miraflores Locks, with the rate of one lockage an hour. This capacity is based on uninterrupted lockages and disregards the effect of certain other important factors to be discussed later. It represents the maximum capacity of the Canal under the most restrictive lock operating conditions—that is, during periods of one side operation at Miraflores when under overhaul.

EFFECT OF FOG

Fog is one of the most frequent and serious interruptions of traffic in the narrow reaches of the Canal. The channel between Pedro Miguel and Bohio is subject to dense fog at frequent intervals, especially in the wet season. South of Pedro Miguel and likewise north of Bohio fog seldom occurs.

Both Gatun and Miraflores Locks can operate on a 24-hour basis under favorable weather conditions because both Gatun Anchorage and Miraflores Lake are relatively fog free. Vessels can lock up from the Atlantic to the summit level and anchor safely at Gatun anchorage without interruption, day or night, and similarly at Miraflores Lake. In fog, vessels cannot enter Gaillard Cut because of the danger of striking the bank; but when Pedro Miguel Locks are clear, northbound vessels may lock up to the north approach wall of the locks to await the clearing of fog in the cut. After the north wall is filled to capacity, all north traffic must stop.

Few persons not directly concerned with the control of traffic in the Panama Canal realize the frequency with which fogs affect marine operations. In 1942 there were two hundred and twenty-three fog reports from marine signal stations in Gaillard Cut during traffic hours, but only 118 fog days. Conditions are worst during the wet season—May to December. Fog forms after 9:00 p.m. but normally clears by 8:30 a.m.

The canal capacity is thus reduced from the rated lock capacity at Pedro Miguel to the lock capacity during favorable weather conditions in the cut.

During periods of long fog duration the capacity is less than the previously assumed minimum of twenty-seven ships a day.

Traffic Restrictions

For certain types of vessels, the Panama Canal has strict regulations requiring one way traffic in Gaillard Cut, known as Clear Cut Rules. Ships laden with explosives, oil tankers, unwieldy vessels, ore ships, large warships, and largest merchant vessels are dispatched only when Gaillard Cut will be clear of vessels to pass. For reasons of safety, traffic in the opposite direction is delayed while they transit the cleared cut.

As a result the capacity of the Canal is still further reduced, particularly when the traffic does not arrive at times convenient for safe and prompt locking. The physical layout of the Pacific locks prevents any compensatory routing of vessels in groups that would overcome these delays.

Accidents in the Panama Canal

In connection with the writer's duties when he was Captain of the Port, Balboa, Canal Zone, the records of all accidents in the history of the Canal that had been formally investigated were examined and indexed. The revelations were impressive. The most serious were found to be lock accidents, groundings, collisions, and accidents resulting from steering gear failure. The study showed that they tended to occur at definite places, and these danger spots were determined.

Of a total of one thousand and thirty-six accidents formally investigated between January 13, 1922, and July 13, 1942, three hundred and ninety-three were lock accidents (Table 2). The most serious were caused by striking the sharp corners of the lock wing walls or ramming them.

TABLE 2.—Number of Lock Accidents, Panama Canal, Formally Investigated Between January 13, 1922, and July 13, 1942

Lock		Entering	In chambers	Departing	Total
Name	Number of chambers				
Gatun	3	87	41	24	152
Pedro Miguel	1	79	15	44	138
Miraflores	2	56	28	19	103

Groundings in Gaillard Cut are feared more than any other type of accident. When a large vessel strikes the rocky bank, the hull may rupture and the vessel may sink and close the Canal. The records are notable especially for the increasing record of sinkings; out of a total of seven sinkings in the 24 years from 1919 to 1942, five occurred in the final 6 years.

Collisions in the relatively narrow waters of the Canal are likely to be serious —especially in Gaillard Cut where the banks are rocky. During this same period there were fifty collisions in the canal channels, of which Gaillard Cut contributed twenty six, causing one out of four sinkings.

Steering gear failure is always on the mind of a master when his ship is in close waters. This has been one of the most prolific causes of groundings and

collisions. Of the one thousand one hundred and eighteen accidents during this same period, one hundred and ten were due to this cause. Steering gear failure in Gaillard Cut makes grounding or collision almost inevitable (Table 3). Obviously, the cut is the most serious danger spot in the Canal.

TABLE 3.—ACCIDENTS CAUSED BY STEERING GEAR FAILURE; JANUARY 1, 1919, TO JULY 19, 1942

Year	Atlantic Sea Level		Gatun Lake		Barbacoas to Gamboa		Gaillard Cut		Mira flores Lake (collisions)	Pacific Sea Level		Total
	Groundings	Collisions	Groundings	Collisions	Groundings	Collisions	Groundings	Collisions		Groundings	Collisions	
1919	..	..	..	..	1	..	..	1	..	..	..	2
1920	..	..	..	..	..	..	..	1	1	..	..	2
1921	..	..	..	..	..	..	1	..	..	..	..	1
1922	..	..	1	..	..	..	1	1	..	..	..	3
1923	..	..	1[a]	..[a]	..	..	4	..	..	1	..	6
1924	1	..	..	1	2	2	3	1	..	1	..	11
1925	1[a]	..[a]	..	..	2	..	5	1	1[a]	1	2	13
1926	1	..	..	..	..	..	3	..	..	1	..	5
1927	1	1	1[a]	..[a]	1	..	4	..	3[a]	2	..	13
1928	2	..	..	..	..	..	4	2	2[a]	..	..	10
1929	..	..	..	1	..	..	1	..	1	2[a]	..	5
1930	..	..	..	..	..	..	2[a]	..	1	..	1	4
1931	..	..	..	..	1	..	2	1	..	1[a]	..[a]	5
1932	..	1	..	..	..	..	1	..	..	..	..	2
1933	..	..	..	..	1	..	1	..	1	..	..	3
1934	..	..	..	..	..	..	4	..	..	..	..	4
1935	..	..	..	..	..	..	..	..	..	1[a]	..[a]	1
1936	1[a]	..[a]	..	..	1	..	2	..	..	..	..	4
1937	1	..	..	..	1	..	1	1	1[a]	..	..	5
1938	..	..	..	..	..	..	..	..	..	..	..	0
1939	..	..	..	..	..	..	2	..	..	1	..	3
1940	..	..	..	..	..	..	1	..	..	2[a]	1	4
1941	..	..	..	..	..	..	1	..	..	..	..	1
1942	..	1	..	..	..	..	2	..	..	..	..	3
Total	8	3	3	2	10	2	45	9	11	13	4	110

[a] Grounding or collision not specified for one case.

The failure of steering gear is not by any means the only cause of serious accidents. Another is the phenomenon known as bank suction,[4] experienced by vessels moving in narrow channels or close to walls. Wall or bank suction often causes grounding as vessels depart from locks or as they pass salients in Gaillard Cut. Usually suction causes a movement of the ship's stern toward the closer bank, center wall, or salient. If the pilot is unable to break the resulting sheer, the vessel may get out of control, strike the wall or bank, or ground on the opposite bank.

In a study by the Panama Canal in 1939, the positions of many groundings were plotted. They tended to occur at bends after the turns were completed, and in some relation to the degree of curvature. The larger turns had the larger number of groundings.

The subject of groundings has not been exhausted. It is a large field for a detailed study that should be undertaken—leading to a scientific determination of necessary changes in alinement of the channels.

Lock accidents have demonstrated the need of modifying the wing walls of the locks so as to eliminate the sharp corners at the lock entrances. The

record of lock accidents and groundings shows that the separation of the Pacific locks into two structures has caused more accidents than would have occurred had the canal plan provided a summit level terminal lake on the Pacific analogous to Gatun Lake on the Atlantic.

The perspective of a quarter of a century of marine operation shows that the separation of the Pacific locks into two structures, the location of Pedro Miguel Locks at the south end of Gaillard Cut, and the failure to create a commodious summit level terminal anchorage on the Pacific, are the great marine operational errors in the planning of the Panama Canal.

The 1939 Third Locks Project

Canal authorities have looked forward to increasing the capacity of the Canal for many years and have made several studies of the subject. However, not until August 11, 1939, was construction of the Third Locks Project authorized by Congress, in those months of hectic activity preceding World War II.

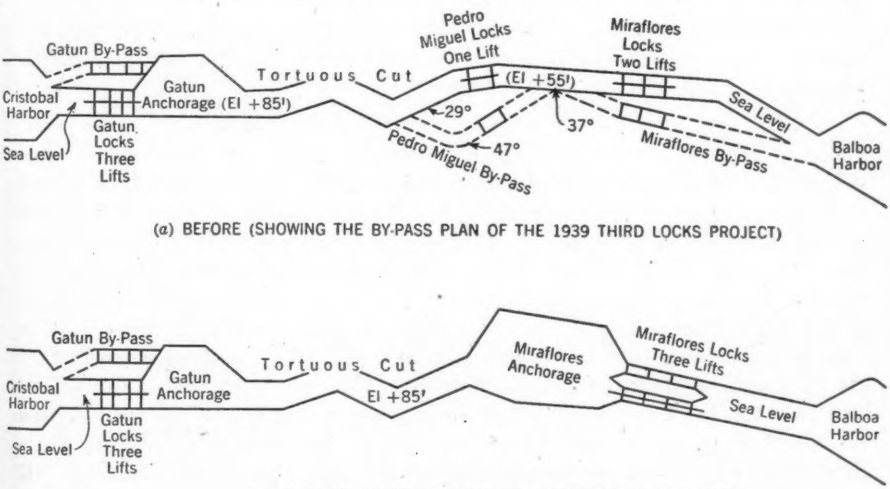

Fig. 2.—Effect of Adopting the Terminal Lake Plan

The purposes were to increase the capacity of the Canal, to permit the transit of large naval vessels, to attain a greater security from bombing attack, and to facilitate conversion from a lock type canal to a "sea level" canal.

The earliest plans for a third set of locks placed the locks alongside the existing structures. The approach of the war and the resulting desire to disperse lock structures because of the danger of bombing caused the adoption of a plan in which the new locks were placed at a distance from the present structures and by-pass channels connected the new locks with the main channels, as shown diagrammatically in Fig. 2(*a*). Except for the segregation of the new locks, the 1939 Third Locks Project represented no fundamental change in the canal plan. It was in principle an acceptance of the present canal arrangement.

Its completion would have perpetuated the bottleneck at Pedro Miguel and would have delayed indefinitely any chance of solving the navigational problems of the present Canal.

Instead of improving and simplifying the Canal, part of the project added certain features dangerous to operation. The proposed new by-pass channel at Pedro Miguel would intersect the present channel near Cucaracha at an angle of 28° 59′. North of the proposed new Pedro Miguel Locks there would be a turn of 46° 17′ in the by-pass channel; and, in Miraflores Lake, still another turn of 37° 30′.

The experience of a quarter of a century has demonstrated that the proposed by-pass channel north of Pedro Miguel would be definitely dangerous, and that it should be abandoned. This channel will not simplify or improve the operation of the Canal; on the contrary, it will complicate the existing situation. The turns of the new channel and the intersection of the new cut with Gaillard Cut would become new foci of accidents; create the most difficult marine operating problems; and make transit of the Canal more hazardous.

Fortunately the suspension of this 1939 Third Locks Project in 1942 occurred at such a stage as to afford an opportunity to re-examine some of its dangerous features.

Sea Level Versus Lock Type Canal

There has been so much discussion of the so-called "sea level" canal as an assumption in the planning of the evolution of the ultimate Panama Canal that an examination of this proposal is essential before focusing attention on the fundamental marine problems that should be solved. The idea is not new. The alluring prospect of the "Strait of Panama" is an ancient historical conception that has had great public appeal. This idea has even been symbolized in the Canal Zone seal which shows a Spanish galleon sailing through Culebra Cut into the waters of the Pacific. Statesmen have made many eloquent speeches setting forth the assumed advantages of the sea level canal over the lock type canal. Yet, in spite of all the rhetoric, a lock type canal was adopted mainly through the efforts of the late John F. Stevens, Hon. M. and Past-President, ASCE. It was completed by the late George W. Goethals, M. ASCE.

In a recent comparative study of the marine features of the sea level and lock type canals, it was assumed that the sea level canal would follow the same general route as the present Canal; would have approximately the same form; would contain a tidal lock at Miraflores; and possibly would have an anchorage or mooring basin north of Miraflores. In effect, such a waterway would not be a sea level canal but a tidal level lock canal.

The conclusions as to the effects on navigation were that the low level lock canal would:

(1) Extend the length of hazardous channel from 7.69 to 31.18 miles;
(2) Increase the number of critical curves (20°+) in the hazardous channel from 2 to 12;
(3) Increase the total curvature in the hazardous channel from 129° to 564°;

(4) Probably extend the channel length subject to fog;
(5) Probably curtail operations during fog periods;
(6) Require the use of ship mooring stations;
(7) Extend the collision, grounding, and steering gear failure area considerably;
(8) Increase the number of transverse streams;
(9) Reduce pilots' vision;
(10) Increase the time required for transit;
(11) Complicate the traffic control problem;
(12) Subject the Canal to the dangers of great floods in the Chagres Valley; and
(13) Reduce the general navigability and operational convenience.

There is only one appreciable marine operational advantage for the "sea level" canal and that is the elimination of the Atlantic locks and the consequent reduction of lock accidents. In comparison, the operational advantages of the lock type canal are overwhelming.

To Solve the Marine Operating Problem

The principal marine operating problems of the present Canal have been described. The way to overcome them is to remove Pedro Miguel Locks from their position at the end of Gaillard Cut; to create a large summit level anchorage in an elevated Miraflores Lake; and to concentrate all Pacific locks near Miraflores in continuous lifts. These changes automatically correct most of the operating problems. This High Level Terminal Lake Plan (Fig. 2(*b*)) is fundamental from the operational standpoint. It will supply the best canal for handling ships.

The land contours of the Miraflores Lake basin are suitable as an impounding perimeter for a terminal lake on the Pacific and will require relatively small dams and dikes. There are several possible lock arrangements. Of these the best marine arrangement is the one in which the present Miraflores Locks are abandoned and all the Pacific locks are placed on a new site that will utilize in someway the excavation for the by-pass locks at Miraflores (Fig. 2). The general layout and relationships of this terminal lake, as of May 9, 1946, are shown in Fig. 3.

The summit level anchorage (Fig. 4) is of greatest marine interest to those charged with the operation of the Panama Canal. It contemplates nine 300-yd berths and thirty 200-yd berths, to be obtained by dredging certain areas. The areas of the several berths, classified as to depth, are as follows:

Depth (ft)	Area (sq miles)
20	0.23
30	0.13
50	0.38
Navigation channel	0.29
Total	1.03

The number of berths in this anchorage should be sufficient for the present traffic even without dredging.

The essential points of the Terminal Lake Plan for the improvement of the Canal are the removal of the bottleneck Pedro Miguel Locks from their posi-

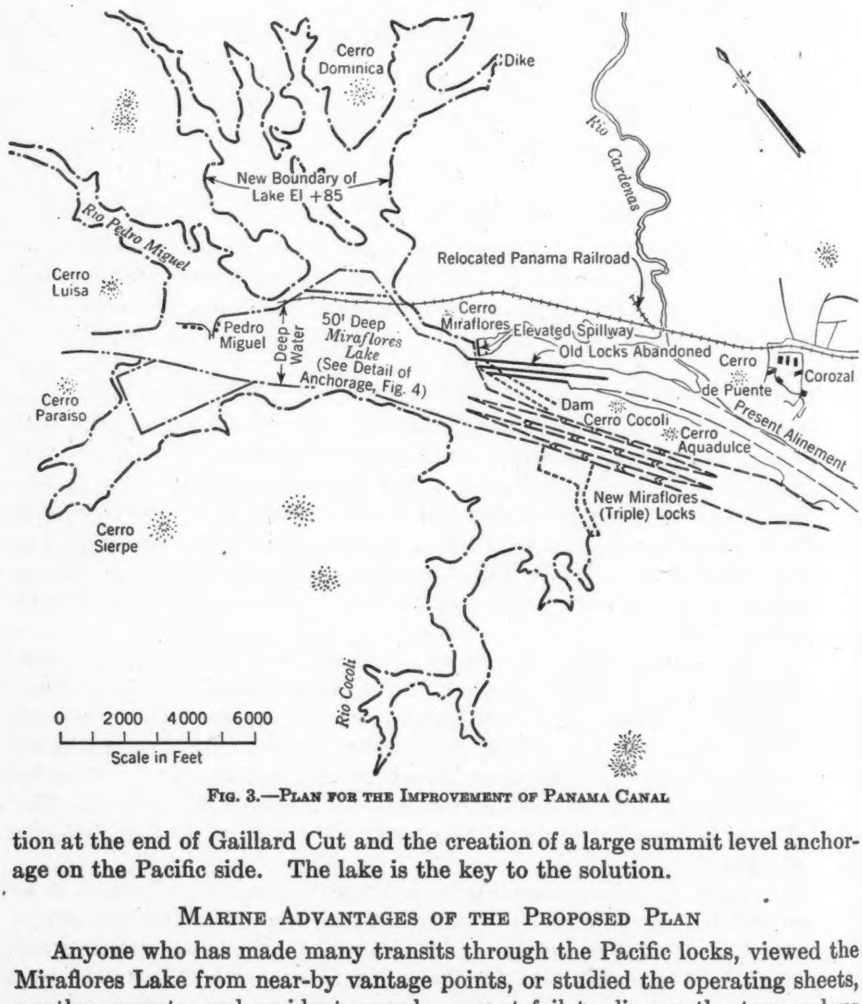

FIG. 3.—PLAN FOR THE IMPROVEMENT OF PANAMA CANAL

tion at the end of Gaillard Cut and the creation of a large summit level anchorage on the Pacific side. The lake is the key to the solution.

MARINE ADVANTAGES OF THE PROPOSED PLAN

Anyone who has made many transits through the Pacific locks, viewed the Miraflores Lake from near-by vantage points, or studied the operating sheets, weather reports, and accident records, cannot fail to discern the tremendous advantages of the Terminal Lake Plan. Among its marine advantages are that it:

(1) Improves and simplifies the operation of the Canal;
(2) Increases the capacity of the Canal;
(3) Extends the useful life of the Canal;
(4) Increases the summit level water storage by 50% to 75%, and the watershed by 37.4 sq miles;

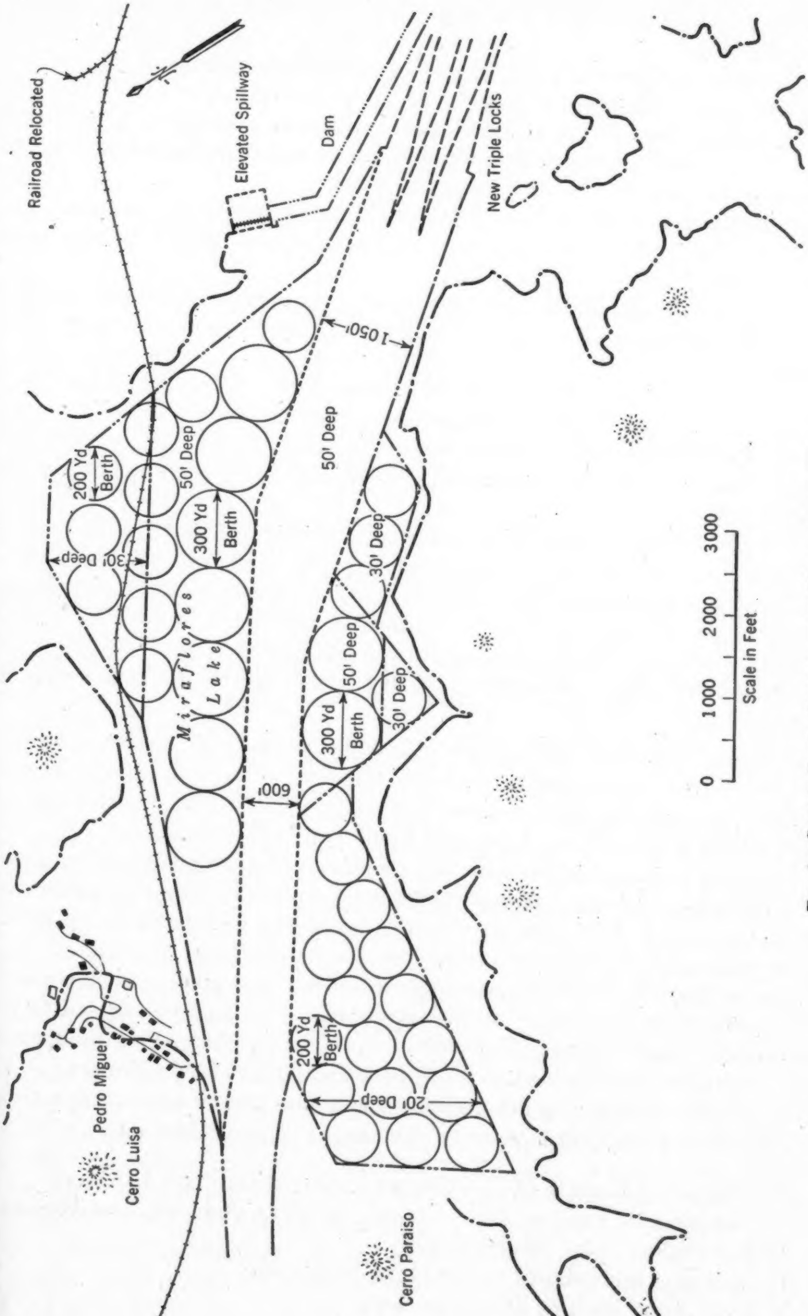

FIG. 4.—SUMMIT LEVEL ANCHORAGE IN MIRAFLORES LAKE

(5) Provides a summit level anchorage between Miraflores and the entrance to Gaillard Cut;

(6) Makes the operation of Pacific locks independent of fog;

(7) Simplifies the problem of dispatching transit traffic;

(8) Avoids the three large turns and consequent groundings in the 1939 Third Locks Project channel between Cucaracha and Miraflores which would necessarily be traversed by the largest ships;

(9) Eliminates one lock from the Pacific lock system and the hazards in Miraflores Lake during the approach and departure of vessels to and from Pedro Miguel Locks;

(10) Reduces the number of expected lock accidents at the Pacific locks;

(11) Removes lockage surges in Gaillard Cut as factors controlling depth of channel;

(12) Simplifies the operations of the Pacific locks;

(13) Reduces the channel maintenance operations;

(14) Reduces the time of transit about 1 hour;

(15) Enables a better distribution of Dredging Division equipment in event of slides;

(16) Increases the safety of transit especially for large war vessels;

(17) Enables a better handling of transit traffic in the event of slides;

(18) Eliminates the lock silting problem in the present Pedro Miguel Locks;

(19) Improves ship handling conditions in Gaillard Cut because of the elimination of surges and the increase in channel depth; and

(20) Removes the dangerous traffic bottleneck of the Panama Canal at Pedro Miguel.

Other operational advantages could be added to this list. So far as known there are no operational disadvantages to this plan. The marine advantages of the Terminal Lake Plan are so extensive that this plan will have a tremendous appeal to the United States Navy and to all merchant shipping.

The main engineering problems will be involved in the construction of the necessary locks and dams. A study of the topography shows that the configuration of the land is favorable for the creation of the terminal lake by dikes. Geological reports indicate favorable foundations for locks and dikes. The engineering features of the plan have been examined by engineers and have been given preliminary approval. All of them would have to be subjected to intensive and detailed study. The cost of the Terminal Lake Plan should not differ materially from the revised cost of the 1939 Third Locks Project.

On the other hand, it is not desired to minimize the problems that will be encountered and that will have to be overcome. Among these are:

(1) Inherent difficulties of changing an approved plan now in effect;
(2) Relocation of sections of the railroad, highways, and pipe and cable lines;
(3) Removal of Pedro Miguel Locks;
(4) Elevating the spillway and dams at Miraflores;
(5) Foundation work at Miraflores; and
(6) Maintenance of canal traffic during construction.

Nevertheless, the navigational superiority of the Terminal Lake Plan to the 1939 Third Locks Project on the Pacific end of the Canal is so overwhelming that it should be adopted even at considerable additional cost. It is the plan that will meet the marine operating requirements of the Panama Canal. It will make possible the construction of additional sets of locks at each end of the Canal. It should be the plan for the ultimate canal.

Historical Perspective

One of the first questions that is likely to be raised after this discussion is, "Why were these fundamental ideas not presented before this year?" The answer is that they were, but they were presented differently and by men without marine operational experience.

In recent years Ralph Z. Kirkpatrick, former Chief of Surveys of The Panama Canal, saw the weakness in the present canal arrangement and submitted suggestions. His main purpose seems to have been to combine the Pacific locks into one structure. His plans were not backed with the force of operating experience and were not adopted.

Before Mr. Kirkpatrick there was the late Maj. Gen. W. L. Sibert, M. ASCE, the builder of Gatun Locks. He wanted to place all Pacific locks between Cerro Cocoli and Cerro Miraflores in one structure as at Gatun. Although he had an excellent grasp of the needs for traffic, his main thesis was economy of construction. His plan was investigated by a board which reported favorably. The report was referred to the President of the United States who decided against adopting the change in the canal plan because the Pacific locks had been started; because it would have meant a delay in completion date; and because any modification would have given the enemies of the Canal an opportunity to seize it as an evidence of weakness in the lock type canal at a time when a political attack could have endangered the completion of the Canal.

Before General Sibert there was Mr. Stevens. In 1906 he proposed the combination of all Pacific locks into one structure near Cerro Aguadulce with a summit level terminal lake formed by a dam between Cerro Aguadulce and Cerro de Puente. He was a transportation man and understood the operational implications of his proposal. Unfortunately, his investigations did not establish the existence of suitable foundations for lock structures. Also during Mr. Stevens' time there was the late William Gerig, M. ASCE, who developed the same idea independently of Mr. Stevens.

Still earlier was the proposal of the French engineer Adolphe Godín de Lépinay at the Paris (France) Congress of 1879. He had worked on the Isthmus and knew the problems that would face canal builders. With a plan of unbelievable simplicity he advocated creating large artificial lakes about 80 ft above sea level at each end of the canal with dams as close to the oceans as permitted by the configuration of the land, and connecting these lakes by locks with the sea level sections of the Canal. The problem then would have been simply one of joining the lakes by digging a channel across the continental divide. This is properly termed the high level terminal lake conception. M. de Lépinay probably was motivated by control of the Chagres River and the reduction of excavation, but he emphasized the navigational advantages of

his plan. The conception of this plan has brought an enduring fame to its author.

M. de Lépinay's idea was not adopted until many years later, in 1906, when the adoption of the high level canal with a dam and locks at Gatun was secured mainly through the efforts of Mr. Stevens. Because there was no provision for a terminal lake on the Pacific side, the Canal as completed in 1914 was only a partial realization of the fundamental conception of the Canal as a marine operating unit. The Pacific sector of the Canal when opened for traffic did not conform to the requirements of the full de Lépinay conception.

The Present Issue

The Panama Canal is again in an era of decision. A vast store of operating experience, not available to the early canal builders, is available for guidance.

When the Canal was constructed, engineering considerations were the chief bases for decisions. Now marine operational requirements rather than engineering problems are the factors that should govern decisions. Just as events forced the relocation of Bohio Dam to Gatun, to form Gatun Lake, the time has come to eliminate the Pedro Miguel Locks and Dam and to concentrate all Pacific locks near Miraflores to form a high level Miraflores Lake.

Primarily the purpose of this paper is to present a historic ideal of the Panama Canal improvement in its modern conception, fortified by thirty years of marine operations. Discussion of these points should clarify the whole problem: No claim for engineering or construction sufficiency is made or intended.

The issue is clear. The solution of the marine operating problems of the Panama Canal consists of (1) the physical removal of Pedro Miguel Locks from their position at the end of Gaillard Cut; (2) the creation of a large summit level terminal lake north of Miraflores Locks for use as an expansion chamber for traffic; and (3) the construction of all Pacific locks in single structures at Miraflores. That plan should equip the Canal for ages to come. Those who bring it about will bestow a tremendous service on the naval forces of the United States and on the shipping of the world. They will be the real modernizers of the Panama Canal. They will achieve the rare distinction that will rank them with the builders of the Panama Canal.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

PAPERS

SEA LEVEL PLAN FOR PANAMA CANAL

TO PROVIDE MAXIMUM SAFETY AND UNLIMITED CAPACITY

By J. G. Claybourn,[1] M. ASCE

Synopsis

National interest and security demand that the Panama Canal be streamlined, in order to secure at the same time maximum safety for operation in war as well as peace and unlimited capacity for shipping. The best method of insuring these improvements is to convert the canal, in approximately its present alinement, to sea level operation.

Supporting such a thesis, this paper proposes a step-down method of deepening and widening the channel, without interrupting traffic and including the possible utilization of mammoth machines for extraordinarily deep dredging. The major problem, to handle floods and runoff on the Atlantic slope, would be solved by building diversion channels and tunnels where needed, supplemented by large longitudinal barrier dams. The flow would be controlled by storage reservoirs and spillways, leading runoff to tidewater independently of the sea level channel.

Incidental problems are discussed, including control of slides, power development, protection in war, and tidal locks. Only enough history is presented to provide a minimum background. Costs are estimated as on the order of \$1,121,000,000 to \$1,910,000,000 and construction time as from 12 years to 20 years—all depending on the methods adopted.

This paper is condensed from "Streamlining the Panama Canal for Maximum Safety and Unlimited Capacity," which was presented before the Construction and Waterways divisions of the Society on January 16, 1946, in New York, N. Y. The views expressed are personal and have no official status relative to the Panama Canal. A copy of the original paper has been filed in the Engineering Societies Library[1a] for further reference and details.

Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by July 1, 1947.

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Introduction

Engineers are familiar with the general layout of the Panama Canal—its three locks at Gatun near the northern end leading to a summit level of 85 ft, its lock at Pedro Miguel on the south side of the Continental Divide connecting with an intermediate level, and its two locks at Miraflores reaching to Pacific tidewater. The distance affected by the proposed conversion to sea level, between Gatun and Miraflores, is approximately $35\frac{1}{2}$ miles. The general features of the Canal are shown in the map, Fig. 1.

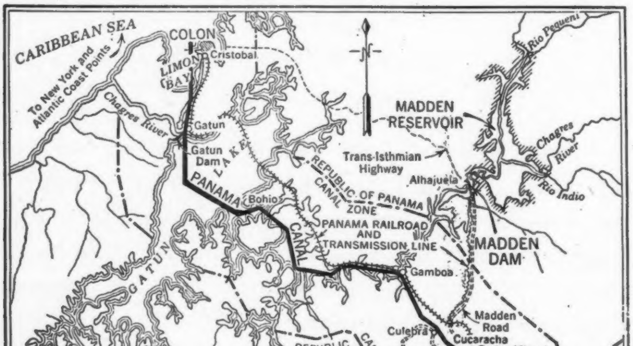

Fig. 1.—Canal Zone and Vicinity

Construction experiences in building the present lock type canal have been tremendously complicated; but they will greatly simplify solution of the vast problems of future development by application of the same close study and coordination. All other construction problems were decidedly secondary to the tremendous task of excavation—piercing the Continental Divide, the greatest obstacle to canal construction. In like manner the amplification of the present Canal, for maximum safety and utility, will be largely a matter of excavation. Questions of economy, time, and defense, coupled with lack of experience and reliable information, precluded earlier realization of the type of canal considered by many as most desirable—a canal without obstructions, or the sea level type.

The controversy of lock versus sea level type (the Battle of the Levels) has been waged virtually from the Canal's inception. In spite of successful operation for more than thirty-one years as a lock canal, it now is imperative

to study seriously the manner of securing, not only maximum capacity, but, more important, maximum safety in case of war.

Early Beginnings

Many volumes have been written on the historic and economic significance of the fabulous Panama country, dating back to the time of Bastidas and Columbus, through Balboa and the Spanish influence over a period of centuries. In the days of the California gold rush and later in 1852 when Lt. Ulysses S. Grant crossed the Isthmus, the United States became intensely interested in the canal project which persisted to its completion. Meanwhile the French, headed by Ferdinand de Lesseps—the builder of the Suez Canal—started a sea level canal at Panama, changed to a lock type, and failed in both, due to financial considerations.

The memorable trip of the battleship *Oregon* around South America in 1898 focused the attention of the American public on the strategic importance and defense needs of an isthmian waterway. In succession there were two early American commissions to handle the construction—the first with John G. Walker as chairman and John F. Wallace, Past-President ASCE, as chief engineer; and the second with Theodore P. Shonts as chairman and John F. Stevens, Hon. M. and Past-President, ASCE, as chief engineer. The problems were critical, particularly the determination of the type of canal. With this in mind a Board of Consulting Engineers was created on June 24, 1915, by executive order of President Theodore Roosevelt. His instructions in part stated:

> "There are two or three considerations which I trust you will steadily keep before your minds in coming to a conclusion as to the proper type of canal. I hope that ultimately it will prove possible to build a sea-level canal. Such a canal would undoubtedly be best in the end, if feasible, and I feel that one of the chief advantages of the Panama route is that ultimately a sea-level canal will be a possibility. But while paying due heed to the ideal perfectibility of the scheme from an engineer's standpoint, remember the need of having a plan which shall provide for the immediate building of a canal on the safest terms and in the shortest possible time * * *."

The Board of Consulting Engineers, failing to reach a unanimous agreement, presented two reports on June 10, 1906. The majority report, signed by eight members, favored a sea level canal. The minority report, signed by five members, favored a lock canal, with a normal lift of 85 ft; it was concurred in by the Isthmian Canal Commission, with one member dissenting, and also attained the support of Mr. Stevens, the chief engineer. Congress then passed an act requiring a lock canal, which became law by signature of President Roosevelt on January 29, 1907.

At that time the waterway was vulnerable only from land or water attack, hence the security features referred to were principally confined to those inherent in making the structures themselves safe from an operational standpoint. In the matter of time, the capacity of new machinery assured completion of the larger American lock canal in about the same interval as that required for the French company's proposed lock type—or ten years. Because of the prohibitive cost of the sea level waterway as evaluated at that period,

the uncertainty of success in the minds of many leaders, and the unknown and unsolved problems which were later encountered but finally mastered, it is generally conceded that the choice of type at the time was amply justified, leaving to later generations any corrective measures deemed necessary.

So great was the progress under the new chairman and chief engineer of the third and final Commission for Construction—Col. George W. Goethals, M. ASCE,—especially in excavation including dredging, that it became possible to put the first ship through on August 3, 1914—with the canal channels in Culebra Cut cleared to full width and navigable depth of 32 ft.

Problem of Slides

Slides were troublesome and costly during dry excavation, but not until after the Canal had been opened to navigation did they attain major importance and become calamitous. Even more important, they were disastrous in neutralizing the Canal's primary purpose—as America's greatest single instrument of defense and offense in wartime.

It was discovered during dry excavation that, whenever the bottom of the cut pushed up, an equal displacement took place in the adjacent banks, caused by the unequal distribution of pressure and in direct proportion to the varying elevations of the adjacent banks above the bottom. Obviously, "* * * if the height of these banks were reduced, the movement would decrease; and if reduced sufficiently, would cease entirely." [2] A contrary view developed—that, by allowing material to move into the cut, the minimum amount would be handled, resulting in a natural slope of the material in addition to reducing cost.

The first assumption was correct. The lack of proper slope of the banks was substantiated as one of the primary causes of slides in the cut, to which was later added the second primary cause for slides—that is, lack of seepage and drainage control. These two causes furnished a well-defined formula for combating, arresting, or preventing slides.

Early it was known that parts of Gaillard Cut were particularly susceptible to landslides, requiring relatively flat slopes, and that the surface must be provided with proper drainage—the latter being considered only a contributory cause. However, because of the failure of the French company and because of immense political and economic pressure brought to bear for early completion of the Canal, a more or less uniform slope throughout the cut section was assumed, and a minimum of drainage was provided. Furthermore, many stretches as originally excavated did stand up, saving temporarily at least the cost of securing a more desirable theoretical slope. The opening of the Canal to traffic was thus expedited, with ever-increasing returns; and the slope problem, of questionable proportions, was left to be solved by the maintenance forces.

In the deep cut sections where slides are definitely to be expected, the procedure of attack is that of working to a predetermined slope in a widening and deepening improvement by removing the high banks immediately bordering the old slope, first sluicing and bulldozing off the overburden followed by mining the open face toward the cut to depths up to 75 ft, with berm widths of

[2] "Annual Report of the Governor," The Panama Canal, Fiscal Year Ending June 30, 1916, U. S. Govt. Printing Office, Washington, D. C., p. 31.

approximately 100 ft. Thus, the frontal step is kept one lift below the adjacent one behind, and this procedure is continued until the predetermined slope line is reached. By this method the objectionable superimposed weight is removed, and simultaneously a new and flatter step slope, nearly conforming to the final angle of repose, is obtained and subsequently retained throughout the operation. Furthermore, the new slope carries much of the blasted material directly to the dredges below. In these operations, up to 15 tons of 60% sodium nitrate dynamite have been used on the highest banks of Gaillard Cut, which are most susceptible to slides, without causing any earth movement except that predetermined in the mined area. Therefore, it is reasonable to conclude that bombing of the canal banks would not result in an appreciable slide movement.

After twenty years of actual experience, a formula for proper slopes in Gaillard Cut has been determined:

From El.+40 to El.+90—slope 1 on 1
From El.+90 to top rock—slope 1 on 3
From top rock to top slope—slope 1 on 5

These slopes are definitely safe for any of the material involved; they are based on nature's great test laboratories—the banks themselves.

The second great development for preventing slides is a systematic plan of seepage and drainage control along both banks of the cut. It consists of diverting all drainage directly into the Canal at suitable places, where rock banks are firm, and, where they are not, of providing permanent works to hold the banks.

Some of the most formidable structural breaks or deformation slides have occurred during the dry season. The presence of impounded water in depressions relatively near the slide areas was apparently given very little consideration by engineers or geologists. Isolated areas still remain to be drained, but the final system for the entire cut area will be postponed, pending definite conclusions as to ultimate development plans in the way of widening and deepening the cut. Possibly some of the slide areas may be extended in the future, and additional bank breaks may be expected because of insufficient slope and lack of proper drainage.

The practicability of conversion of the lock type canal to sea level at Panama has been questioned at times on the score of slides. Results to date definitely indicate that conversion to sea level is entirely practicable in so far as slides are concerned.

Channel and Other Improvements

Immediately after the great slide menace was brought under control, the Canal began to serve its primary purpose as a great instrument of offense and defense in World War I. It increased the effectiveness of American naval and transportation fleets. The tremendous aid to the war effort was apparently taken as a matter of course by the general public.

With the tremendous development of the airplane, every step possible was taken for the Canal's defense by the time of the outbreak of World War II.

Even so, it was generally conceded that the lock type canal continued to be vulnerable to a determined aerial attack.

The necessity of improving and enlarging harbors and channels became evident almost immediately after the Canal was opened. Such projects consisted mainly of widening or deepening the channel and easing the bends. In December, 1923, a special Channel and Harbor Improvements Board submitted a series of twelve channel and harbor improvement projects, covering various points in the Canal difficult to navigate, and also the deepening of the Pacific entrance to a ruling depth of −50 ft mean sea level, to accommodate the deepest draft vessels at any stage of the tide.

In 1933 sixteen additional projects were proposed to eliminate closure by slides and to increase capacity, involving the widening of Gaillard Cut to a minimum channel width of 500 ft and deepening it an additional 5 ft, or to a bottom elevation of 35 ft, precise level datum. The advantages claimed were: Channel blocks caused by slides would be cured by applying stable bank slopes and installing proper seepage and drainage control; directional or one-way traffic would be eliminated; speed of ships through the Canal would be increased, thus decreasing the cost to shipowners and raising canal revenue; hazards to shipping would be decreased by improved navigational conditions; ill effects of surges and necessity for the construction of a costly surge basin would be eliminated; and all the improvements would contribute toward conversion to sea level when or if such procedure is determined.

Three of these projects combined in one, known as project No. 13, have been under construction since 1934, in the section most susceptible to slides. The work is now (1946) more than 50% completed.

Concurrently with the development of canal channels, studies were started for a dam at Alhajuela, later known as Madden Dam, previously contemplated by the French. It was completed in February, 1935. The primary objectives in order of importance were: (*a*) To control drainage of the Chagres River into the Canal in time of freshet; (*b*) to maintain Gatun Lake at a normal elevation of 85 ft during the dry season, thus increasing the capacity of the Canal by providing additional lockage water; and (*c*) to provide additional electric power for operation of locks and other uses.

Third Locks Undertaken

Studies looking toward expanding and improving the Canal were authorized by Congress in 1929 and again in 1936. As a result of the latter investigation, Governor C. S. Ridley, in February, 1939, recommended that locks be started within ten or twelve years on the basis of commercial requirements alone. This project, costing $277,000,000, was to incorporate locks of greater dimensions than formerly. In consideration of defense, they were located at a distance from the old structures, entailing additional excavation to provide channels connecting the new sites with existing waterways. In August, 1939, Congress authorized the construction of Third Locks substantially in accordance with this plan, but stipulated as the purpose, "* * * in the interest of defense and interoceanic commerce."

Early estimates for performing necessary excavation were based on a six-year work program—from July, 1940 to June, 1946. Then, because of the likelihood of war, the over-all schedule was accelerated to permit use of the new locks by June, 1945, thus reducing the construction program to a five-year period. However, war developments caused practically all the work on the Third Locks to be terminated in May, 1942, except wet excavation, which was carried on as a backlog of work.

Considering the developments that have been made since Pearl Harbor in the range and size of planes and the destructiveness of their bomb loads, and the further developments that may be expected, the dependability of the lock type canal as a wartime transportation and supply route is very doubtful. It is therefore necessary to consider streamlining the Canal in such a manner that this service at all times shall be available.

Possible Modifications in Third Locks Construction

Following general cessation of Third Locks construction, various changes were suggested in the original plan when and if construction is resumed. The main reasons were: (*a*) If conversion to sea level is considered necessary, and is undertaken in line with the plan of lowering the lake by steps, as contemplated in the report of Governor H. Burgess, M. ASCE, for 1931, the project will have been advanced by the removal of Pedro Miguel locks; (*b*) suitable design can be incorporated in new Third Locks and existing locks at Miraflores to facilitate conversion; and (*c*) certain improvements in operation will have been gained for the existing lock type canal in the interim.

The general scheme conforms to that proposed in 1907 (except for the addition of Third Locks) by the late Maj. W. L. Sibert, M. ASCE, member of the Isthmian Canal Commission, and later advocated by a board of consulting engineers in February, 1909. It was also given consideration by the Interoceanic Canal Board of 1931, the general object being to concentrate all three flights of Pacific locks at Miraflores, similar to the arrangement on the Atlantic side at Gatun; and to create, in the words of the consulting engineers,

> "* * * a Pacific Terminal Lake * * * more than a square mile in area, immediately above the locks, in which ships going in either direction can come to anchor to wait for the lifting of night fogs in Culebra Cut, if east bound, or for any other convenient purpose. This terminal lake would be of great convenience in connection with fleet lockages * * *."

Other advantages claimed for the Terminal Lake scheme are: The elimination of Pedro Miguel locks, the bottleneck at the south end of the cut; the elimination of surges in the cut; the elimination of the construction of third locks at Pedro Miguel and the excavation of a second cut, between Miraflores and Gaillard Cut; and the reduction of traffic time and accident rate caused by the present separation of the locks on the Pacific side. Of the several schemes presented, only one plan seemed reasonably economical and therefore acceptable if conversion to sea level is to be long delayed—namely, that of constructing an additional chamber on the north end of Miraflores Locks, of appropriate height for the summit level, and the construction of all new third locks at Miraflores at the side originally designated for two lifts.

It will be apparent, however, that all the disadvantages enumerated in the lock type canal will be corrected and the additional advantages claimed for the Terminal Lake scheme will be obtained, automatically, upon conversion to sea level. In addition, many advantages of greater, more vital, and more far reaching importance will have been gained by such conversion, for

(1) All high lift lock bottlenecks will have been removed.

(2) Surges in the cut, created by lockages, will have been eliminated.

(3) No additional channels to additional locks will have to be excavated.

(4) The traffic time and accident rate, as affected by these artificial barriers, will have been reduced.

(5) The narrow cut section will have proper side slopes from an increased bottom width. This will allow ships to be maneuvered to the new full 500-ft bottom width instead of an actual 200-ft navigable width, as at present, because of the present vertical bank slopes (10 vertical to 1 horizontal) which prohibit ships from utilizing the full 300-ft width now available.

(6) Electric power requirements for the Canal will have been reduced to navigational aid demands.

(7) Most important, the necessary artificial storage of lockage water, and, concurrently, the annual water shortage fear, for the lock type canal operation, will have been eliminated and with it the greatest danger of prolonged closure of the Canal by enemy action.

Danger of grounding ships in channels of 500-ft bottom width, with bank slopes and increased widths at turns as recommended, cannot be taken seriously when compared to the number of accidents which have occurred under conditions existing in the present lock type canal. Little trouble has been experienced with the transit of average size ships, even in the comparatively narrow channels of Gaillard Cut section, except as may be inherent in the power plant or steering gear of such vessels. The difficulties have been with the large, deeply laden vessels termed "bad handlers" and may be attributed to various causes, such as poor design; underpowered, insufficient rudder surface; uneven trim; and the great displacement of water in relatively confined channels. In the latter case, the hull is too close to the bottom or to the comparatively vertical banks, and thus prevents sufficient accessibility of water to the propeller and rudder—referred to in the "Pilot's Handbook" as bank suction.

From all available data, it appears that fogs were as prevalent before canal construction as at the present time and possibly more so. Furthermore, fogs will probably remain as a navigational problem, should the lock type canal be converted to sea level. The safety of navigation in the cut section, due to fogs, unquestionably would be increased by its widening, permitting any cross wind to aid in fog dissipation. It is possible that even the comparatively slight fog and occasional rain squall that menace navigation may be neutralized by the application of scientific discoveries produced during the war, such as radar or other devices not yet disclosed, which will permit at least directional or one-way traffic during these comparatively short stoppage periods, permitting all-out navigation of the channel under these conditions.

In any event, fogs throughout the cut section are not nearly of such import to the shipping capacity of the Panama Canal as is the one-way traffic required by narrow channels during daylight operations. The widening of the cut section would more than make up for the capacity lost as a result of fog.

Proposal for Conversion to Sea Level

The multilock canal at Panama has been in successful operation for thirty-one years. It is agreed that, if a lock type plan is to be retained, it can be improved, if desired, toward the optimum capacity by the addition of locks as required, incorporating the Terminal Lake Plan, but at great expense. There always remains, however, the element of vulnerability of the lock type in time of war and this is increased each time a lock, a dam, a spillway, or other control work is added. Furthermore, great additional cost is involved in widening the cut each time a lock is added, to maintain proper navagational conditions. Otherwise undesirable currents would increase in proportion to the withdrawal of lock water from the summit level; and, moreover, the resulting increased cost of augmenting the necessary water supply is evident in the form of more dams and control works, each in turn increasing the Canal's vulnerability. Even mechanical methods of supplying water for the lock type canal must be considered because the deepening and widening process, for the present summit level and lock structures, is limited, as is the storage of water in reservoir systems at higher elevations.

On the other hand, enlargement for easier navigation or for augmentation of the capacity to the optimum by conversion to sea level operation is simple and relatively inexpensive, because such improvement will be made as traffic demands, with no change in flood or other control works necessary.

It is believed that practically all engineers familiar with the Canal's construction, maintenance, or operation considered the lock type favorably until the destructiveness of aerial warfare was so forcefully demonstrated in World War II. This raised serious doubts concerning the feasibility of protecting such a canal against an aerial attack.

It seems entirely possible that the lock canal could be put out of business without even endangering the personnel conducting the attack. A sea level canal could not be destroyed by such methods. Once the water in the summit level is dissipated, the lock canal could conceivably be out of commission for the duration of a war. As against this, Navy strategists apparently consider it an essential that transit of all vessels between the Atlantic and Pacific oceans shall be rapid, easy, and safe for defensive and offensive operations in any global war.

The feasibility and practicability of converting the Canal to sea level form has received approbation from the Board of Consulting Engineers in 1906, and from Governor Burgess of The Panama Canal and the Interoceanic Canal Board of 1931.

The Board of Consultants, 1906,[3] admonished:

[3] "Report of the Board of Consulting Engineers for the Panama Canal, 1906," U. S. Govt. Printing Office, Washington, D. C., 1906, p. 39, paragraphs 1 and 2.

> "That it is possible to turn any lock canal which it has considered into a sea level canal without interrupting the traffic upon it * * *"

but such conversion is

> "* * * impracticable from a financial standpoint until the traffic should have so increased as to tax the capacity of the lock canal, or until other good and sufficient reasons exist for such a change."

The good and sufficient reasons have become most apparent. To quote a very applicable statement of the Supervising Engineer, E. E. Abbott:[4]

> "If the hypotheses that a lock canal cannot be made impregnable and transits without delay of many months are essential to the security of the United States, are accepted, it must be assumed that a sea-level canal program must be the ultimate solution * * *."

Step-Down Plans for Conversion

The general plan proposed for the conversion from lock to sea level canal follows that described in Governor H. Burgess' report of August 4, 1931, to the Secretary of War, and the excavation scheme is based on a revision of the plan by S. B. Williamson, M. ASCE, formerly Division Engineer of the Pacific Division, later a member of the Interoceanic Canal Board in 1931. This method (Fig. 2) permits conversion without interference to traffic by excavating one half, or 250 ft, of the channel to an additional 30-ft depth, while traffic is confined to the other half and then by interchanging operations to the opposite side—the latter operation to be followed by the removal of the locks at Pedro Miguel and the upper locks at Gatun. The same procedure is followed in the second lift, after which the top flights of locks at Miraflores and the second flights at Gatun would be removed. Finally, by the same procedure, the last lift would be removed down to sea level. Coincidentally, with the excavation of channels and removal of locks, the diversions, necessary dams or barriers, and control works would be installed progressively at the appropriate periods.

The primary reasons for prior completion of Third Locks as a forerunner to the execution of this plan, in whole or in part, are fourfold: (1) To insure continuous traffic through the Canal during conversion; (2) to permit additional lockages for excavated material and movement of construction plant, when and if required; (3) to provide for lock overhaul during construction; and (4) to allow for normal increase of traffic during conversion. An additional advantage of the step-down scheme is that lifts may be made piecemeal, with intervals between them if required for any reason.

Studies for Flood Control of the Chagres

Flood control of the Chagres River and its tributaries is considered the most difficult problem to be solved in effecting conversion to sea level as well as in providing a permanent flood control system thereafter. This feature was accomplished for the lock type canal in the creation of Gatun Lake. The French company's sea level plan to accomplish this purpose provided a dam across the Chagres River above Gamboa and a spillway into an East Diversion, which

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[4] "The Hypothetical Sea-Level Project," by E. E. Abbott, memorandum to the Engr. of Maintenance, the Panama Canal, Records Bureau, Balboa Heights, December 2, 1944.

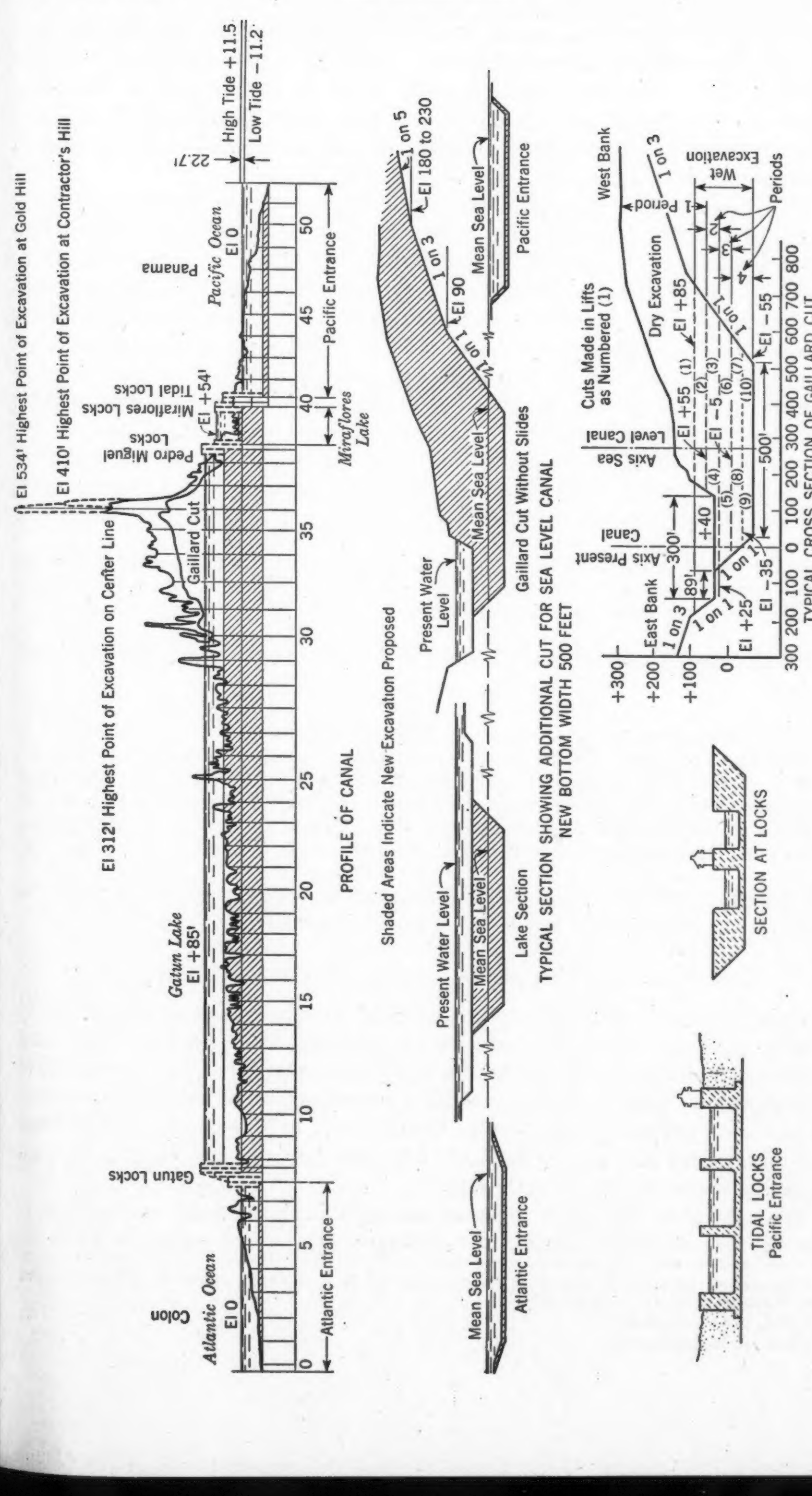

FIG. 2.—CONVERSION TO SEA LEVEL OPERATION, SHOWING PROFILE, SECTIONS, AND ORDER OF PROCEDURE

eventually discharged its waters into Manzanillo Bay at the Atlantic end (Fig. 3). In addition, the canal channel itself was to be used in part for flood control. The tributaries from the south were to be handled by the West Diversion, discharging into the Lower Chagres, and thence into the Atlantic Ocean. These diversions more or less paralleled the canal channels and at some points were joined.

The Board of 1906 planned[5] a solid masonry dam across the Chagres River at Gamboa, with a top elevation of 180 ft mean sea level. The highest flow line of the reservoir thus formed was 170 ft; it included an area of 29.47 sq miles. Through this dam 15,000 cu ft per sec was to be discharged into the canal channel with a minimum depth of 40 ft, and a minimum wetted cross section of 8,000 sq ft. For the size of canal prism planned, this would have resulted in a current, flowing in one direction, of $1\frac{1}{4}$ miles per hr—a negligible quantity in so far as navigation is concerned, discounting the effects of fresh water mixing with salt water. The board assumed that, with a flood of the proportions of 1879, in which 65,000 cu ft per sec inflow was recorded at Gamboa for a period of 48 hours, there would accumulate in Gamboa Lake, 8,640,000,000 cu ft of water, which is:[6]

> "* * * that portion of the volume of the lake included between the water surfaces at elevations 159 and 170 feet M.S.L. Furthermore, * * * 15,000 cubic feet per second would discharge * * * the 1879 flood in 8.7 days."

Furthermore the board stated that the capacity of the flood control of such a lake:[7]

> "* * * between water surfaces of 108 and 170 feet M.S.L. is sufficient to take the aggregate discharge of three times the maximum average 48-hour flow of the 1879 flood, without any water escaping through the regulating sluices of the dam; or the volume between 128 and 170 feet M.S.L. will hold three times the flow of such a flood, if a uniform discharge of 15,000 c.f.s. be permitted concurrently through the regulating sluices."

This board, therefore, concluded that the capacity of Gamboa Lake would be ample for the exigencies of any flood on the Chagres River.

New Plan for Chagres Control—Eastern Tributaries

Under the new plan herein proposed (Fig. 3) diversions and control works therefor shall be absolutely independent of canal channels and thus shall positively eliminate any ill effects that floodwaters might have on the Canal, or on navigation. This plan also provides a suitable reservoir system to accomplish the same purpose as the present Gatun Lake, in allowing floods to spread over large areas and thus dissipate their force, finally discharging through regulating works on the Atlantic side.

The control of the Upper Chagres and tributaries formerly running into it from the east must be handled in two stages: (1) During conversion and (2)

[5] "Report of the Board of Consulting Engineers for the Panama Canal, 1906," U. S. Govt. Printing Office, Washington, D. C., 1906, pp. 42–45.

[6] *Ibid.*, p. 44, paragraph 2.

[7] *Ibid.*, p. 44, paragraph 3.

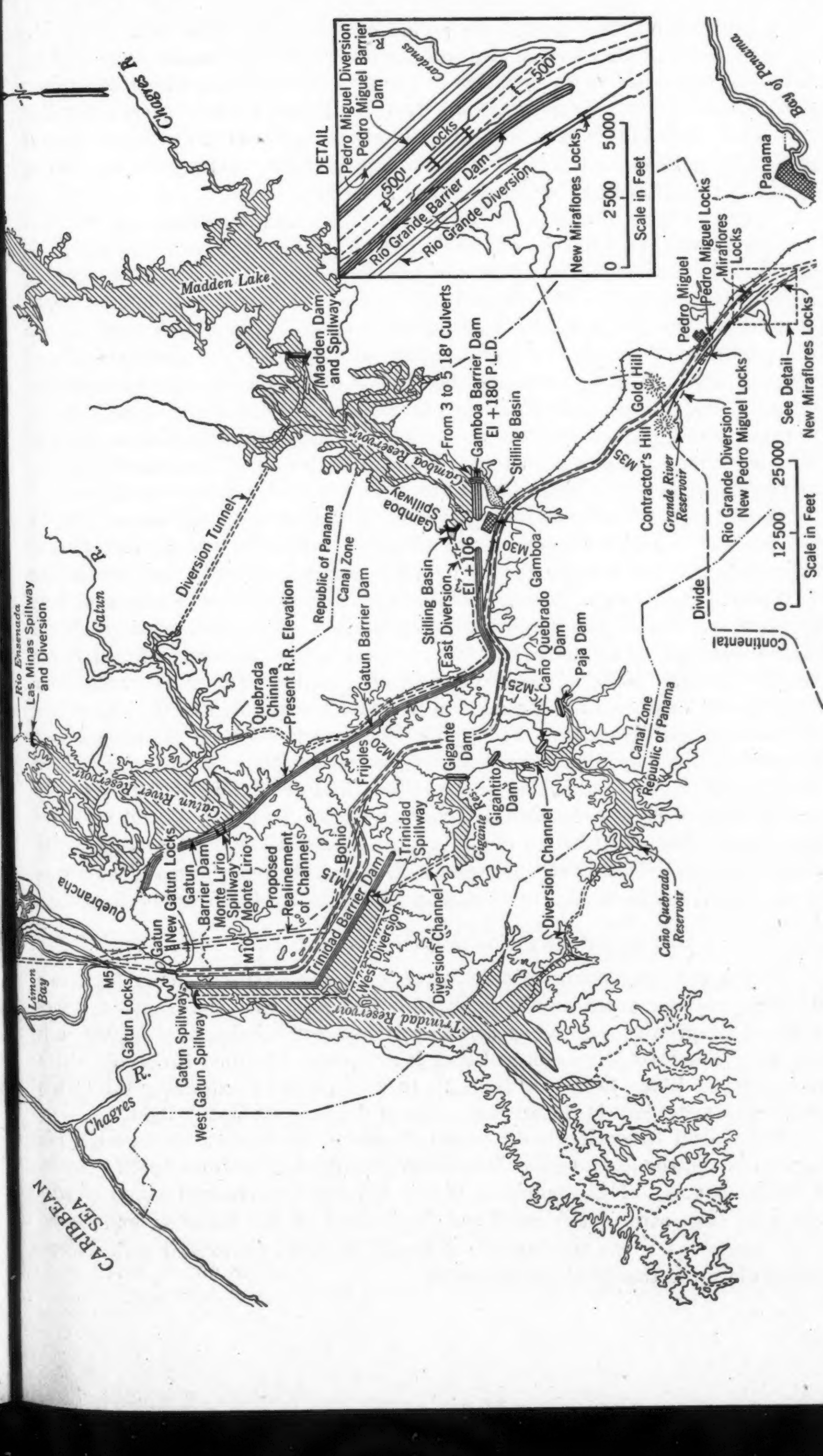

FIG. 3.—FLOOD CONTROL BY DIVERSION, SHOWING PROPOSED FACILITIES TO PROTECT SEA LEVEL CANAL

after conversion. The conversion period envisions a barrier dam across the Chagres River at Gamboa, of such proportions that it cannot be destroyed by bombing and raised to an elevation of 180 ft mean sea level, with a maximum flood line of 170 ft. Conduits at low elevation through this barrier, controlled by valves, with a maximum discharge of approximately 30,000 cu ft per sec will serve (*a*) for flood control; (*b*) for water supply during conversion; and (*c*) as safety factors afterward, for flood control if desired.

In this plan a spillway is also provided, with a maximum flow line of 170 ft mean sea level, and a maximum discharge of 50,000 cu ft per sec emptying into an East Diversion extending to Quebrada La Chinina, approximately 13¼ miles northwestward. The channel will have a bottom width of 350 ft, a depth of 20 ft, slopes of 1 to 1, and a bottom elevation 76 ft mean sea level, at the Chagres River end. It will carry 50,000 cu ft per sec at a mean velocity of approximately 6.8 ft per sec on a slope of 0.4 ft per thousand. Detailed studies would be required to determine the optimum cross section and slope. If the channel is designed for part of this floodwater, the remainder may be handled by a diversion tunnel connecting the Upper Chagres and Gatun rivers, or may be permitted to flow into the Canal through the culverts already mentioned.

In addition, a continuous barrier dam from Gamboa to Quebrancha will be provided, of sufficient proportions to withstand bombing, with an elevation of approximately 106 ft mean sea level at the Gamboa Spillway, and continuing 10 ft above the diversion flow line to a point opposite Quebrada Chinina with an approximate elevation of +85 ft, and then to Quebrancha at approximately the same elevation. This design would provide a safety freeboard against floods of approximately 10 ft, corresponding with that for the barrier dam at Gamboa. The material required for these barriers would be provided from excavations of channels and diversions. It may be necessary to shift the Gamboa Reach Channel of the Canal southward slightly, to reduce the cost of excavation of the East Diversion Channel, as well as to provide sufficient width for the Gatun Barrier Dam between the East Diversion and this channel. It will be noted that the outflow of 50,000 cu ft per sec in the East Diversion, combined with other methods enumerated, compares with the maximum volume of 15,000 cu ft per sec contemplated in the 1906 plan for regulating the Gamboa Reservoir.

Discharge and Controls—Eastern Tributaries

During the conversion period it is proposed also to use Madden Dam as a flood control and water supply device in conjunction with the Gamboa Reservoir. The spillway is to be operated at a maximum discharge of 50,000 cu ft per sec. The East Diversion will thus join the high Gamboa Reservoir with a lower Gatun River Reservoir (Fig. 3), to be formed by extending the Gatun Barrier Dam across the Gatun River arm of the existing Gatun Lake.

The Gatun River Reservoir would discharge its floodwaters through Rio Ensenada and thence, into Las Minas Bay through a Las Minas Spillway and a 6,000-ft channel. The dimensions of this spillway and channel would be adequate to take care of the combined floodwaters of the Gamboa and Gatun River reservoirs. The top flow line of the latter would not exceed +75 ft mean sea level during the construction period.

When the sea level canal is opened to traffic, it may be found advisable to have the spillway regulating works, at Gamboa and Las Minas, operate at a lower level than during the conversion period, to provide for greater flood cycles. In any event, they would provide the same outflow to retain the advantages of the reservoir system. Working models would indicate the proper installation so that alterations would not be necessary at the end of the work.

During the conversion period, it is possible to take advantage of a feature proposed by the Hydraulics Section of the Special Engineering Division in 1941. This would provide additional lockage water during the critical or lower stages of conversion, when elevations of the smaller sized Gatun Lake will fluctuate more violently because of reduced area. The installation of a regulating spillway at Monte Lirio (through the Gatun Barrier Dam) would permit storage waters to be conducted from the Gatun River Reservoir into the present Gatun Lake, over a 300-ft overflow spillway, at elevation +45 ft mean sea level, with a discharge of 60,000 cu ft per sec at a maximum water surface elevation of 59 ft. Obviously, the storage in this reservoir may be increased by giving greater range to the spillways at Monte Lirio and Las Minas.

Other features of the plans of the Hydraulics Section provide for a dam at Gamboa, 95 ft above the stream bed or approximately at 135 ft mean sea level, 4,000 ft long, with a control spillway 400 ft long, a crest elevation of 105 ft mean sea level, a discharge capacity of 190,000 cu ft per sec, and outlet conduits having a capacity of 30,000 cu ft per sec with water at crest elevation. This scheme provides a discharge into a stilling basin, and thence directly into the Canal. Its disadvantage is that destruction of the dam would result in a flood of sufficient magnitude to interrupt navigation; however, considering the great increase in cross section of the prism channels, 30,000 cu ft per sec might be discharged into a 500-ft channel without materially influencing navigation, discounting any ill effects produced by discharging fresh water into salt water. As may be noted, this proposal revives the same scheme that was advocated by the Majority Board of 1906.

A complete study of all methods must be made before any one method, or a combination of methods, can be recommended for adoption. Working models of appropriate size will be of the utmost value and importance—to determine a balanced control of flood and feeder problems, to determine the maximum amount of water that can be discharged into the Canal safely, to uncover omissions, or to correct erroneous conclusions. In any event, the independent flood control as described would appear to be the safest from the viewpoint of aerial attack, because any damage to embankments can be repaired quickly and cheaply, and the ill effects of introducing waters of varying quantities and densities directly into the Canal will have been eliminated.

Provision for Electric Power

It is possible to secure some electric power from Madden Dam, from the proposed spillways at Gamboa and Las Minas, and at the outlet of the West Diversion (to be described subsequently). Nevertheless, it is considered decidedly inadvisable to depend on such power after once starting to reduce the

elevation of the summit level, for such a system cannot function to greatest efficiency as a source of power, requiring high head and full basin capacity, and at the same time, at a moment's notice, provide for storing floodwaters, requiring a low head or a comparatively empty basin. It is, therefore, proposed to provide for all necessary electric power requirements by installing a number of permanent diesel or steam plants, widely separated, but at convenient points or by using sources entirely independent of the Chagres River watershed, thus obtaining the greatest possible protection by this dispersion.

Obviously, it will be necessary to give the highest priority to the creation of the flood control system for the Upper Chagres watershed (above Gamboa and Gatun Barrier dams) in the over-all construction program so that it will be ready to function immediately the present Summit Lake level is reduced. Such procedure will provide accessible dumps for judiciously utilizing excavated material from the adjacent channels and the diversion, from the start of the job. The essential features of the flood control system for the Lower Chagres watershed must be completed by the time the present Summit Lake area is reduced appreciably below +55 mean sea level datum.

It is proposed that the Madden Dam, after it has served its purpose as a flood control factor during the conversion period, will be made inoperative by the removal of its control works. This structure will then remain only as a highway bridge across the Chagres River.

Questions of Vulnerability and Drainage

Those favoring other basic plans for the Panama Canal will immediately emphasize that the control works of this scheme are also vulnerable. This is true, of course, but they are separate from the Canal itself; and, even if the spillways or regulating works were to be destroyed, no damage to the Canal would result, for the drainage system would still function. Furthermore, as the 1906 Commission reported, the water surface in the reservoirs "* * * would be depressed immediately after any flood, low enough to receive any subsequent sudden flow, which might possibly occur."

Under this procedure all critical control works will have been removed by the time sea level channels are placed in commission and the Chagres River will have been reduced to normal original flow. There will also be the additional safety factors of the large Gamboa and Gatun River reservoirs.

Drainage into the cut section, which is relatively small, would be allowed to enter the Canal as at present. In reducing to sea level, the area thus drained would not be enlarged. Therefore, control works, which must be built in any event, would conduct this water into the Canal without scour and with very little, if any, more serious effect on shipping than at present, which is nil.

Control of Lower Chagres—Western Tributaries

Control of floodwaters from tributaries of the Chagres entering from the west side and below Gamboa will be accomplished by an improved counterpart of the method proposed in the report of the Board of Consulting Engineers of 1906. A system of reservoirs would be created (Fig. 3) and the flow of streams would be reversed by ditching, to discharge through low saddles near their

middle or headwaters. For example, the Caño Quebrado and the Gigantito would be reversed and diverted into the headwaters of the Trinidad; and the headwaters of the Trinidad and Gigante, in turn, would be diverted into the basin of the Peña Blanca Marsh, through which the West Diversion will have been excavated, and thence into the Old Chagres river channel through the new West Gatun Spillway and so out to sea.

To the eastward of the West Diversion, between it and the channel proper, the Trinidad Barrier Dam would be constructed more or less paralleling those channels extending south from the present Gatun Dam. Barrier dams at the mouths of the Gigante, Gigantito, Caño Quebrado, and Paja would, in turn, divert these streams as described, thus sealing off all fresh water drainage into the Canal from the old tributaries of the Chagres formerly running into it from the south. Material for dam construction would be obtained from excavating the West Diversion Channel, the adjacent canal channels, and the cut, thus furnishing a disposal area for scow as well as for hydraulic dredge operations. Runoff from the small drainage area on the canal side of this continuous barrier dam would not affect shipping.

Additional lockage water to be provided from the Trinidad Reservoir will, no doubt, be necessary during the excavation of the last lift of the canal prism—a critical period of conversion. For this purpose a temporary Trinidad Spillway would be introduced through the Trinidad Barrier Dam system.

The advantages of the reservoir-diversion-barrier dam scheme are multifold: It prevents interference to traffic by invulnerabilities to bombing and by sealing off freshet water from entering navigable channels; it eliminates the objectionable effects of turbulent currents on shipping as a result of fresh water mixing with salt; it provides an easily accessible spoil area for both dry excavation, by rail, or wet excavation, by hydraulic methods or scow dump; it provides the necessary reservoir capacities and insurance against any floods reaching the Canal; it provides more flexibility for flood control than obtains in the lock system and also safeguards against silting fully as well as the lock canal; on the east side it permits the removal of all bridges on the Panama Railroad, reduces grades, and provides strategic rail and road communication across the Isthmus that is the shortest practicable; it is indestructible; and it is entirely in the Canal Zone, under American control.

Drainage South of the Divide

To take care of the relatively small amount of drainage waters south of the Continental Divide, it may be necessary to re-establish the old diversion on the west bank to accommodate the Rio Grande watershed to the westward of the Canal, discharging into the Pacific. On the other hand, model studies might indicate the feasibility of simply installing control works where the Rio Grande meets the Canal, and also where its largest tributary from the west (the Cocoli River) meets the Canal. Similarly, the tributaries eastward of the Canal could be provided for by a short diversion, discharging below the present Miraflores Locks, or through regulating works where these small branches meet the Canal.

Favorable Features

Because of all these provisions, a flood of the proportions of those of 1879 or October, 1923, could be faced without misgivings. This reservoir system is more effective than Gatun Lake because of a division of the total Chagres River watershed, with a potential discharge through regulating works more than twice as great as is now possible at Gatun Spillway, supplemented by the lock chambers at Gatun and Pedro Miguel. The increased capacity of this flood system, therefore, provides safeguards beyond any reasonable expectancy of flood increase. The maximum safety feature during conversion, obviously, must be sacrificed; but this danger is lessened tremendously the sooner it is accomplished immediately after a war, when the element of war damage is most remote.

The system proposed has greater advantages in guarding the Canal against silting than obtain in the present Gatun Lake.

If conversion is not delayed until the indefinite future, the Third Locks can be integrated into the scheme in a greatly simplified form, resulting in an appreciable decrease in cost. It is probable that no new locks would be built at Pedro Miguel and the new locks at Gatun could begin service at the level of the middle chamber. The lowering of the lake level would progress so rapidly that the upper chambers of the new locks would scarcely be required if constructed.

Deep Dredging Plan

An alternative plan for conversion, recently (1944) proposed by Supervising Engineer, E. E. Abbott, deserves serious consideration. In this "Deep Dredging Plan," all channels in the summit level would be dredged to grade, and flood control works and diversions would be completed, while maintaining the present summit level at an elevation of +85 ft. Obviously, these are great advantages in that problems of hydraulics are largely eliminated, except those inherent in the present lock system and the completed project. Flood control systems identical to those for the step-down plan would be provided; but no doubt savings in execution would be realized. The entire control system would be completed by the time the excavation is finished and the construction of Third Locks would not be necessary. The capacity of the present lock type canal will be sufficient to take care of all traffic until conversion is completed, if the work is started before, say, 1948. This is because only three commercial vessels and a few naval vessels are too large for the present locks.

In this plan enormous dredges of great power, capable of excavating to depths of 135 ft, will be necessary. A plant of such power and capacity is considered definitely practicable. Great savings are indicated in the final costs for this plan, if it proves practicable. However, a combination of the step-down and deep dredging plans would possibly meet conditions more satisfactorily than would either one exclusively.

If the primary lift of the step-down plan is executed first, followed by the last two lifts under the deep dredging plan, practically all benefits of both will have been attained. The following advantages are then indicated: (*a*) Conventional type dredges may be employed by increasing the power, length of

ladders, spuds, and dipper handles; (*b*) the proposed flood control system, incorporating the high level reservoirs for the construction period, would make available the necessary lockage water lost in reducing the present summit level by one lift or step; (*c*) construction of one Third Lock at Miraflores and one Third Lock at Gatun, of simple design with one lift, equivalent to the two lifts as of present design, would reduce over-all costs materially and simplify conversion; and (*d*) the solution of other hydraulic and construction problems claimed for the deep dredging plan would still obtain.

Tidal Locks

After many years of debate the necessity of tidal locks in a sea level canal at Panama is still an open question. As a precautionary measure, sufficient funds for such tidal locks have invariably been included in estimates. In most discussions the important point has been overlooked that, if a sea level canal is deemed essential, it should be built whether tidal locks are needed or not. Such locks would affect only the navigation conditions in the Canal. Regardless of the results of the investigations to determine if tidal locks are necessary, the problem is entirely subordinated to the essential decision of converting the existing Canal to a sea level canal.

The installation of tidal locks would be made for the sole purpose of improving nagivation conditions in case tidal currents are sufficiently strong to constitute a hazard to navigation. The majority report of the Board of Consulting Engineers stated:[8] "It is probable that in the absence of tidal locks, the tidal currents during extreme spring oscillations would reach five miles per hour." Recent calculations, based on the formula by Brig.-Gen. G. B. Pillsbury,[9] M. ASCE, indicate that the maximum velocities may be as high as 6 miles per hr (5.2 knots).

Hydraulic model tests will be necessary to determine the intensities of currents in the Canal and the measures that may be undertaken to reduce their effect on navigation. A decision can then be made with respect to the tidal locks. If it becomes established that tidal locks are required, they should be designed so that their total cross-sectional area is at least equal to the cross-sectional area of the canal prism. Then, should one of the locks be placed out of operation in such a manner that uncontrolled tidal flow would take place through it, the other locks can be opened and the velocities at the lock section will not be greater than those in the channel. A smaller cross section at the tidal locks would result in increased velocities and a sudden change in the surface profile.

A tidal lock, which might best be placed at Miraflores (Fig. 3), would be a relatively insignificant structure in comparison to the present Panama Canal locks. It would have to be designed for operation under a maximum head of about 12 ft for control by sector gates that can be operated against flowing water. Should the lock be placed out of operation, transit through the Canal might be limited somewhat, but it would not be suspended as is the case if the

[8] "Report of the Board of Consulting Engineers for the Panama Canal, 1906," U. S. Govt. Printing Office, Washington, D. C., 1906, p. 56, paragraph 2.

[9] "Tidal Hydraulics," by G. B. Pillsbury, *Professional Paper No. 34*, Corps of Engineers, U. S. Govt. Printing Office, Washington, D. C., November, 1939.

present locks were destroyed. Because of their greater simplicity, tidal locks could be repaired with greater ease, in less time, and at much less cost.

Increased maneuverability, if desired, can be provided either during original construction or by progressively widening the channel as regular maintenance after the Canal is in operation. The additional width would not appreciably increase the velocities of the tidal currents and would improve conditions for navigation.

Estimates of Cost and Time

A cursory check of the 1931 estimates for converting the present lock canal to sea level, revised to 1946, indicates that a 500-ft sea level canal channel with all appurtenances, as described, would cost approximately $1,310,000,000. If the conversion is made without constructing Third Locks, this estimate may be reduced by $100,000,000. The estimated cost of the deep dredging plan in which no Third Locks are contemplated is $1,121,000,000.

A change of alinement of canal channels is possible at the north end, in the Gatun Lake section. This would shorten the waterway by 10,000 ft, eliminate a bad turn at Bohio, and shorten the time of transit by 10 min—for an additional expenditure of $25,000,000. The reason for the present alinement, in the French as well as in the American sea level plans, was that, by following the course of the Chagres River, less excavation would be entailed. This condition was also true for the lock canal to a lesser degree.

It is also estimated that an additional $600,000,000 would be sufficient to construct a veritable Strait of Panama, 1,000 ft in width throughout its length, from ocean to ocean. Originally the cost of building a sea level canal was considered prohibitive; nowadays its cost can be considered as only a fraction of what the nation will spend for national defense. At the rate of expenditure in World War II, it would require the equivalent of 5¼ days' cost for a 500-ft sea level channel and about 7⅝ days' cost for a 1,000-ft sea level channel. It is not advocated, however, that the 1,000-ft channel be installed at once, but rather that it be considered as a matter of maintenance when traffic demands.

The cost of conversion will vary with the urgency of the completion date. In the 1931 studies, twenty years was considered a reasonable time requirement. Twelve years will probably be the minimum. The longer period is approximately the span between great wars. Since a depression, or at least a critical adjustment period, inevitably occurs after a war, a job of this magnitude would provide employment for an abundance of available technical personnel, as well as skilled and ordinary labor; and, moreover, as a consequence a great saving in the cost of the project would redound to the government.

No attempt is made to describe the step-by-step procedure in the alteration of locks for the lowering of Gatun Lake. This procedure has been covered, however, in *House Document No. 139*;[10] and modifications have been discussed in a report on *House Joint Resolution No. 143*, prepared in 1941, by the Special Engineering Division.

The cost of operation of both lock and sea level canals would probably be about the same for a number of years, or until all control works governing

[10] *House Document No. 139*, 72d Cong., 1st Session, pp. 35–38.

drainage throughout the Canal had been completed and tested under most severe working conditions and until canal banks had received their initial set. After this there should be a decided reduction in cost of maintenance and operation in favor of a sea level canal, since slides will have been eliminated and since scour from freshly excavated channel banks and runoff from adjacent areas will have been stabilized. Large suction dredges, specially made for the purpose, will be able to cover great channel areas in maintenance, and should show a decided downward trend as to cost, by virtue of large output and relatively close hydraulic dumps, following methods already used at the Pacific and Atlantic entrances for the disposal of hydraulic spoil.

Problems of Maintenance and Operation

In connection with the slide menace, it may be stated that, in the process of widening or deepening the cut section, which must be performed to the westward, the banks will have been reduced to a lower level in many places than at present since the accepted slopes now being applied will cut off the tops of the adjacent hills, intersecting the down slope at the back of these hills. The towering volcanic plugs—Gold and Contractor's hills—through which the Canal now passes at the divide will likewise not constitute a menace, for Contractor's Hill will be removed in the process of widening and Gold Hill will have been cut down to impotency.

Engineers at Panama are no longer working in the dark in excavating through heterogeneous materials or conglomerate strata which make up the geological formations through the cut and other sections of the Canal. For twenty years this deepening and sloping process has been going on in the deepest sections of the cut most susceptible to slides, with positive results.

As an item of disadvantage total curvature (or summation of curves), cannot be taken too seriously, nor held as a valid reason for unsatisfactory navigational conditions in a sea level canal, because additional and adequate widths are provided for maneuverability of ships at the turns. The sea level Suez Canal, whose channels are less than half those proposed at Panama, is ample proof of this statement.

Based on the normal velocity of $7\frac{1}{2}$ knots through Suez, a speed of 10 knots through the proposed uniform 500-ft sea level channels at Panama would be permissible. The time scheduled for the present Canal as a minimum for the passage of ships between the north end of Gatun Locks and the south end of Miraflores Locks—30.4 nautical miles (35 statute miles)—is $5\frac{1}{2}$ hours, or a rate of 5.5 knots (6.38 miles per hr). With a permissible speed of 10 knots through a 500-ft channel, this same distance would be negotiated in 3 hours 5 min or a saving of 2 hours and 25 min.

In Favor of Conversion to Sea Level

Engineers now have the background of experience to build whatever is required at Panama, whether it be a lock type canal of optimum capacity, or a conversion of the present Canal into a relatively invulnerable sea level canal, which, it is believed, will provide unlimited capacity and will be continuously

useful in war and in peace. There are no problems in either case that cannot be solved; it is purely a question of ultimate value to the country in time of war.

In so far as national defense is concerned most engineers conversant with the problem agree that it would serve the purpose better to convert the Panama Canal to sea level than to build a second lock type canal at Nicaragua. The costs would be approximately the same. Other advantages of Panama over the Nicaraguan project have been pointed out very clearly by Gen. Hans Kramer,[11] M. ASCE.

Conversion of the present Canal is not justified from the standpoint of commercial earning power alone; but if it serves its purpose of defense on one critical occasion its cost will have been justified. The relative invulnerability of the sea level type has been demonstrated at Suez; its serviceability, by the fact that it is passing the largest ships efficiently, at low cost and with practically no hazard. The Suez Canal has been blocked by sunken ships during World War II, but only for days, not months or years. Its effectiveness as a dependable world-wide waterway has been demonstrated, even with channels less than half the minimum recommended for Panama.

The sea level type possesses all the advantages from low cost enlargement to unlimited capacity for traffic, in addition to insurance against prolonged interruption of traffic from natural causes or enemy action. The only advantages of the lock canal over the sea level type, at the time of construction, were the lower first cost and earlier completion date. However, this procedure was justified, for it provided a usable canal, which many thought was impossible at the time; and resulted in timely aid to the United States and her allies during World War I, as a great instrument of defense and offense—the main purpose for which it was constructed. The intensity of the critical period has now abated, but the war value of the Panama Canal is continuous. The Canal was not attacked during the two world wars, but it may not be so fortunate another time.

To emphasize the importance of the Canal as an instrument of defense and offense, it is only necessary to state that immediately when war was declared on December 8, 1941, commercial traffic as such stopped or was curtailed because the necessities of war demanded the transiting of vessels of war—troop ships, or supply ships. It is significant that the number of ships, and also the gross tonnage, passing through the Canal was comparable to that of the greatest commercial traffic during peacetime. The monetary value of this service to the United States government, contributory to the success of the war, is inconceivable and incalculable.

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[11] "The Isthmian Canal Situation," by Hans Kramer, *Transactions*, ASCE, Vol. 94, 1930, p. 406.

AMERICAN SOCIETY OF CIVIL ENGINEERS
Founded November 5, 1852

DISCUSSIONS

CAVITATION IN HYDRAULIC STRUCTURES
A SYMPOSIUM

Discussion

By John K. Vennard, John C. Harrold, Jacob E. Warnock, and George H. Hickox

John K. Vennard,[16] Assoc. M. ASCE.[16a]—It is gratifying that that Symposium has stimulated so many civil engineers to place their cavitation experiences before the profession as a whole. The main purpose of the Symposium was to provide a convenient summary of theories, experiences, and references on cavitation. It is felt that the excellent discussions submitted have contributed heavily toward attaining this objective.

Professor Mockmore rightly emphasizes the necessity for care in surface finish to reduce cavitation and pitting. The importance of careful finishing of surfaces has been recognized in the propeller and turbine field for some time; on the other hand, a few tests described in the literature seem to indicate that surface roughness is quite irrelevant. This question is another ramification of the problem that has never been decided conclusively.

Professor Robertson's comment on the "elementary" treatment of cavitation theory is not taken to be a criticism since an elementary treatment was the intention of the paper. Professor Robertson raises the question of the effect on cavitation of internal tensions in a liquid, and thus encounters another unsolved problem of cavitation. Pure, air-free, unagitated water has been shown capable of withstanding enormous tensions for relatively long periods, but obviously such conditions would never be satisfied in engineering occurrences of cavitation. Nevertheless, it is possible that, even in "engineering water," tensions may exist for a sufficiently long time to contribute significantly to the rapid formation of the cavity. Future research may answer this question. Thanks are due Professor Robertson for his neat summary of hypotheses and

Note.—This Symposium was published in September, 1945, *Proceedings*. Discussion on this Symposium has appeared in *Proceedings*, as follows: December, 1945, by C. A. Mockmore; February, 1946, by Joseph N. Bradley; March, 1946, by J. M. Robertson, and Fred W. Blaisdell; April, 1946, by John S. McNown; May, 1946, by James W. Ball, and Fred Locher; June, 1946, by Carl E. Kindsvater, and E. R. Driest; and October, 1946, by Duff A. Abrams.

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[16a] Received August 8, 1946.

phenomena connected with cavity collapse which were not included in the original paper. This summary has served to strengthen the effectiveness of the Symposium.

The question asked by Mr. Blaisdell cannot be answered at the present time. Whether pitting may result from the collapse of cavities out of contact with the pitted area is another question for the research laboratory. Eventually, if this question is answered in the negative (assuming other advances in the knowledge of cavitation), the designer will have a valuable tool for rendering cavitation harmless.

Professor McNown indicates the possibility of dealing with some problems of cavitation caused by flow curvature by assuming a perfect fluid and applying the methods of classical hydrodynamics. Situations where this procedure can be followed, however, are a very small minority of those encountered in engineering practice; it still appears to the writer that, in the large percentage of problems in which cavitation results mainly from flow curvature, pressure reduction is quantitatively unpredictable and its causes are obscured by the complexities of three-dimensional flow. Professor McNown's remarks on a dimensionless approach to the cavitation problem, on the effect of air content, and on the use of the water tunnel have contributed materially to the value of the Symposium and are much appreciated.

Professor Van Driest's observation on the contribution of corrosive action is pertinent. Originally it was felt by numerous engineers that corrosion was the mechanism by which pitting occurred. All evidence now seems to indicate that cavity collapse is the main destructive effect, with corrosion playing a small, usually negligible, part; however, this is not to deny that there may be an effect of corrosion since metals are known to fatigue more rapidly in the presence of corrosive action.

The writer is skeptical of the validity of Professor Van Driest's conclusion concerning tension in a column of flowing water although he cannot offer conclusive refutation. The evidence that there was tension in the liquid at the throat of the constriction is certainly circumstantial; a moving liquid column (presumably containing dissolved air), sustaining tension for the time implied by Professor Van Driest, is inconceivable to the writer. Furthermore, the numerical calculations offered by Professor Van Driest do not appear convincing. The writer suspects that a change in the flow picture has produced a change in the value of K_L which Professor Van Driest has assumed to be constant; if this is the case, the calculations leading to tensions of 770 lb per sq ft and 118 lb per sq ft would be in error.

JOHN C. HARROLD,[17] ASSOC. M. ASCE[17a]—The writer was pleased with the many original ideas, practical suggestions, and additional cavitation experiences presented in the discussions. They should add greatly to the value of the Symposium.

Professor Mockmore's suggestion that care in fabrication is just as important as proper design has also been learned by the Corps of Engineers

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[17a] Received September 10, 1946.

through experience at Norfolk Dam (Arkansas) where, in constructing the sluice gate slots described by Professor Kindsvater (Fig. 64(*a*)), the castings did not match within ½ in. in some cases and had to be ground flush after the dam was put in operation.

Messrs. Bradley and Kindsvater brought out the necessity for distinguishing between the cavitation due to a general lowering of the pressure gradient in a conduit and that due to a localized reduction caused by a discontinuity or sharp curvature in the boundary. Mr. Bradley's example (Fig. 52) showing that, in a straight uniform conduit, cavitation can occur only in the roof of the conduit because the bottom is always under higher pressure than the roof was very interesting.

The remedy described by Mr. Bradley for the low-pressure gradient in the Ross Dam (Washington) by-pass tunnel is essentially the same as that used in the Norris Dam (Tennessee) sluices (Fig. 46). In each case, the downstream end was constricted in order to raise the general pressure gradient in the conduit. Constrictions of the Norris type have been built into the sluices of some of the dams recently constructed by the Corps of Engineers.

Use of the beveled gate slot, described by Professor Kindsvater (Fig. 64(*a*)), has also become common practice with the Corps of Engineers. A general series of model tests is under way (1946) at the U. S. Waterways Experiment Station in Vicksburg, Miss., to attempt to improve the streamlining of gate slots. General tests are also under way at the Experiment Station which have for their purpose improvement in the design of the bottom edge of rectangular slide gates and the determination of the cavitation characteristics of elliptical entrance curves for rectangular sluices.

In reply to Mr. Blaisdell's question about the practicability of baffle piers with the sides cut away to form a water cushion, a model test of just such a baffle pier conducted in the vacuum tank at Carnegie Institute of Technology in Pittsburgh, Pa., in connection with the Bluestone Dam (West Virginia) tests (Fig. 18) will be described. The baffle piers for this test were 6 ft high and 5 ft wide, with a vertical upstream face and sloping downstream face as in the other tests; but the upstream face in this test was flared to an 8-ft width providing a 1½-ft overhang on each side. The upstream face was made convex in plan with a 6-ft radius and the transition from the 8-ft width to the 5-ft width was made with a concave surface, also with a 6-ft radius. The piers were spaced on 8-ft centers and were staggered in two rows as in the other tests. The following is quoted from the report on these tests:

> "The former [above] design was based on the assumption that the destructive effects of cavitation on the sides of the baffle could be eliminated by providing an area, or water pocket, for the formation of eddies in the region where cavitation would likely occur. The front of the baffle was designed in such a way as to direct the jet away from its sides, these being depressed to allow the water to feed in from behind and provide an area of positive pressure against them due to eddy formation. The behavior of this design was as expected, no pockets occurring adjacent to the pier. The jet was deflected away from the sides of the baffle, and the resulting low pressure areas were confined to the water in between the baffles. However, the effectiveness of this 'side-pocket' type of pier in energy dissipation was not as good as for the other types tested for Bluestone Dam."

Although in this particular case the "water cushion" baffles did not prove to be the best solution to the problem, the idea is basically sound and might prove more advantageous in another case. There is one practical objection to these baffles, however—they are structurally less rugged than the streamlined type. The upstream side edges being sharp and relatively thin would be subject to damage by impact from heavy drift or ice. The streamlined baffles have no projecting corners and would be less subject to damage. On the other hand, as stated by Mr. Blaisdell, the streamlined baffles also have a practical disadvantage in that the curved surfaces must be constructed more accurately.

Concerning the remaining two questions raised by Mr. Blaisdell: The writer does not know how the noise level of collapsing cavities is related to the damage caused by them; nor does he know of any instance where the structure, as a whole, failed because of the forces resulting from collapsing cavities. As stated in the writer's paper, sections of armor plating on the sides of the Bonneville (Oregon) gate piers and on the sides of the Gatun (Panama Canal) baffle piers were ripped off due to fluctuating pressures possibly involving the action of cavitation. However, the structures in these instances are of such rugged construction that no tendency toward failure of the structures, as a whole, has been noticed. Perhaps, in time, some such tendency may be observed. However, it is possible that the great width of these piers and the heavy steel reinforcement used in them may cause unit stresses resulting from the fluctuating pressures to be quite low. It is also possible that the forces themselves may not be great because the area exposed to extreme high and low pressures may not be great.

The writer appreciated the more complete theoretical analyses of the cavitation phenomenon and the model simulation thereof given by Professors McNown and Van Driest, and he was grateful that these analyses checked Eqs. 3. Their discussions also brought to light many interesting physical characteristics of the cavitation phenomenon which those not well versed in physics would not realize existed and which should be useful to practicing hydraulic engineers in interpreting cavitation experiences.

The additional cavitation experiences cited by Messrs. Ball, Locher, and Kindsvater should be helpful in future hydraulic designs.

There is one more comment on Professor Kindsvater's discussion which appears desirable. In writing about the reliability of electric pressure cells for measuring pressure fluctuations in the model of the Bull Shoals (Arkansas) sluice gate slots, Professor Kindsvater states that the cells "* * * gave inconsistent results, and these data were discarded." This may appear contrary to the statement in the writer's paper that satisfactory results can be obtained by this method of measurement. However, the statements will not seem contradictory when it is explained that the writer's statement applies to great fluctuations in pressure such as were measured on the sides of the baffle piers in the model of Bluestone Dam whereas Professor Kindsvater's statement applies to small pressure fluctuations downstream from the gate slots in the model of the Bull Shoals sluices. The pressure cells have been found unreliable in the lower range when measuring rapidly fluctuating pressures; and, as a

result, further experimentation is under way (1946) at the U. S. Waterways Experiment Station to determine the causes of this trouble and the remedies for it, if possible. It is suspected that the presence of minute air bubbles in the copper tubing leading from the piezometer openings to the cell may be the principal causes of the difficulty. Apparently, the error is inappreciable with great fluctuations in pressure; but, with small fluctuations, the results are quite inconsistent as Professor Kindsvater states. This leaves hydraulicians at present (1946) with no reliable method of measuring small fluctuations and with the necessity of applying an arbitrary factor of safety to measurements made with an ordinary water manometer.

Since the writing of the original papers of this Symposium (1944), severe pitting, due to cavitation was discovered in the outlet works control towers of Fort Peck Dam in Montana. The outlet works consist of three circular tunnels 24 ft 8 in. in diameter and about a mile long with vertical control shafts near the center. The discharge of each conduit is controlled by a cylinder gate, 27 ft 7 in. in diameter and 8 ft 2 in. high, 55 ft above the invert of the tunnel in the control shaft. The water rises vertically through an annular ring 6 ft 6 in. wide concentric with, and outside of, the 28-ft 1-in. circular shaft. It then flows laterally through six rectangular openings 8 ft 2 in. high and 7 ft 4 in. wide into the shaft; thence downward into a 24-ft 8-in. elbow which deflects the water horizontally into the 24-ft 8-in. diameter tunnel. The cylinder gate controls the six lateral openings and seats on the bottom edge of these openings. The sides and top of these openings are plane surfaces. The sides are radial with respect to the center line of the shaft and the top slopes downward in the direction of flow on a slope of 30° with the horizontal. The bottom of the opening is 6 ft 2 in. thick in the direction of flow and is shaped like the crest of an overflow dam. The sides, top, and bottom of the openings and the inside surfaces of the shaft in the vicinity of the openings are lined with semisteel armor plating. Severe pitting of these curved crests and the shaft lining in the vicinity of the openings has occurred to a maximum depth of $1\frac{1}{2}$ in. The cylinder gates have been operated partly open and wide open for extended periods under heads up to 200 ft (pool to invert of tunnel). In addition, the cylinder gates themselves have been damaged by vibration and a section of the steel liner plating in the 24-ft 8-in. elbow has been torn out. Some of this damage may have been caused by the closing of the air vents in the control shaft, which was found necessary during winter operation to prevent freezing of the spray in the shaft. Very little winter operation is contemplated in the future. If the foregoing conditions continue to exist, it may be necessary to work out a change in design to remedy them. Thus far, repairs have been made in accordance with the original design.

The Corps of Engineers is constructing (1946) a large vacuum tank at the U. S. Waterways Experiment Station, similar to the tank at Carnegie Institute of Technology for the cavitation testing of various parts of flood control structures. The inside dimensions of the test chamber will be 8 ft long by 4 ft high by 4 ft wide. The test chamber has been designed for testing models with or without a free water surface.

JACOB E. WARNOCK,[18] M. ASCE.[18a]—Although the Symposium papers have stimulated a worthwhile budget of cavitation pitfalls to guide the efforts of future designers, the writer is somewhat loath to see the discussion close. He is certain that, if every organization dealing with hydraulic structures and machinery would open its files, as the authors of the Symposium papers have done for their respective organizations, a more comprehensive list of examples of cavitation damage could be contributed. If this were done, the purpose of the Symposium (namely, that of allowing the profession at large to benefit by the experience of its individual members) would be more fully realized.

Of the many discussions submitted only one is in need of direct reply and comment. The discussion by Mr. Abrams covers the writer's portion of the Symposium very thoroughly, and it is unfortunate that much of his discussion is based on misconceptions on his part. He does not concede that cavitation can damage a concrete tunnel lining. Some of Mr. Abrams' comments are concerned with the quality and placing of concrete and are not discussed here because they have no place in this Symposium; and, also, the subject has been fully treated in other publications. Those dealing with the subject at hand are fully discussed herein.

In his discussion of the Boulder Dam spillway tunnel, Mr. Abrams states, "* * * a photograph of a rope is not very satisfying as scientific evidence * * *" of tunnel misalinement. Actually, the misalinement was carefully surveyed. The photograph, however, was submitted because it was a more striking evidence than a contour drawing. Mr. Abrams' statements regarding the misalinement of the tunnels have no basis if the facts are considered. Certainly, the entire lining could not shift after the damage occurred without leaving evidence of such movement. Also, the misalinement indicated by the rope could not have been caused by erosion, since the black waterproofing was still on the surface of the concrete in many places after the damage had occurred.

Mr. Abrams then states, "An official report of the USBR published in 1938 stated that there was no misalinement in the Boulder Dam spillway tunnels." Careful reading of the excerpt chosen by Mr. Abrams (78a) [18b] will show this to be a misstatement:

> "* * * The Boulder Dam spillway system is designed with the expectation of obtaining practically streamline flow in the lower tunnel sections where maximum velocities occur. Comprehensive specification provisions for accuracy of alinement and rigid control of concrete manufacture and placement help to insure this.
>
> * * *
>
> "Since considerable care was taken in designing the shapes of the spillway transitions to eliminate negative pressures or vacuums, it is not expected that erosion will occur due to cavitation."

Nowhere in these excerpts is it stated that there was no misalinement of the tunnels. It is stated (78a) that, "specification provisions for accuracy of

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[18a] Received November 8, 1946.

[18b] Numerals in parentheses, thus: (78a), refer to corresponding items in the "Literature Cited," which appears as the last unit of the Symposium, and at the end of discussion in this issue.

alinement" were made and that "care was taken" in design. These procedures were followed, but in spite of the precautions, the misalinement still occurred.

Mr. Abrams' understanding of "cavitation" does not agree with the accepted definition. He quotes the writer's statement:

> "Actually, the coat of black waterproofing and mineral deposit was intact in many places, showing no effect of direct scouring by the high-velocity water immediately above and below the eroded area."

and interprets it to mean that cavitation could not have occurred. Actually, this is positive evidence that cavitation did occur. Pitting or erosion due to cavitation begins a short distance downstream from a source of low pressure—in this case, the misalinement at the point shown by the rope. No measurable erosion occurred above the misalinement; and this fact constitutes strong evidence that, even with high-velocity flow, if changes in contour are not present, there is no tendency for the high-velocity water to erode the concrete. Apparently, the shearing action of the fluid against the boundary in the absence of abrasive material did not wear the concrete.

Considerable space is devoted by Mr. Abrams to a discussion of erosion of concrete test blocks, using quotations from a USBR report (78) to illustrate his points. The tests selected by Mr. Abrams to illustrate excessive concrete erosion are impingement tests. The jet of high-velocity water was pointed directly at, or at a sharp angle to the test block surface. These tests (78*e*)(78*f*) were run (quoting directly from the report)—

> "* * * in order to determine the erosion produced by such extremely severe conditions * * * tests at a jet angle of 45 degrees and 90 degrees were intentionally extreme or accelerative and have no direct application to the spillways. These tests are enlightening, however, in revealing the resistance of the concrete to severe or abusive treatment."

Nowhere in the Boulder Dam tunnels is the concrete subjected to direct exposure to a jet of water traveling 175 ft per sec and impinging at a 45° or 90° angle. Flow in the Boulder tunnels is such that the streamlines are parallel, or nearly so, to the concrete surfaces. Quoting again from the report (78*g*), and referring to the test blocks,

> "There was practically no evidence of wear or erosion of the concrete on any plane or jointed surface subjected to the water jet at any velocity when the angle between the jet and the block was small."

Mr. Abrams refers to the test jet as "a scattered spray" and then declares, "It is not clear how a nozzle that discharged a scattered spray 4 in. in diameter could form a hole in the concrete * * *." Actually, the "scattered spray" referred to by Mr. Abrams was a concentrated jet of water emanating from a nozzle that tapered from 4 in. in inside diameter to 1 in. in a length of 20 in. The jet impinged on the sample 15 in. from the nozzle with a velocity of 175 ft per sec; yet Mr. Abrams calls this a "mild type of erosion."

In two places Mr. Abrams calls attention to the results of tests on block A-2 and refers to a "hole 1-inch deep and 12 square inches at the bottom." Here he is quoting a typographical error (78*d*) meant to read ½ sq. in. This should have been clear, regardless of the typesetter's omission of a diagonal line, since a photograph of the test block, included in the report, shows the area of the hole to be considerably less than 1 sq in. at the bottom.

In discussing the results of tests on block F-1, Mr. Abrams completely disregards the text of the report which states (78):

> "In blocks having a cylindrical hole or semicircular groove, the apparent wear was inappreciable, except near the point of water exit from the block, see Figures 129 and 130. At those points, substantial cavitation occurred under jet velocity of 105 to 175 feet per second. The cavitation is attributed principally to outlet disturbances accompanying the sudden release of the stream from its constricting channel."

These statements are substantiated by a photograph of the block taken after testing was completed.

In connection with the foregoing discussion of cavitation and erosion, it is interesting to note the relative location of the erosion that occurred on the test blocks. Referring to tests with jet angles of 90° the report states (78*e*), "Areas affected had the shape of an annular ring approximately 1-inch wide around a 2-inch inside diameter circle * * *." Contrary to Mr. Abrams' description, the jet was a solid stream of water about 1 in. in diameter where it impinged on the test block surface; yet, there was no erosion within the area of impingement or within a concentric circle about 2 in. in diameter. All the measurable erosion occurred outside the 2-in. circle. A satisfactory explanation of this phenomenon is not readily apparent when one considers the known facts concerning either erosion or cavitation. Similar erosion has been experienced by others and no explanatory conclusions have been reached. Further experience and knowledge are needed to fully understand this type of surface destruction.

The comparison of model and prototype given by Mr. Abrams is not valid because of his misinterpretation of the model data. Furthermore, the nature of the damage to the prototype cannot be predicted from the model tests since cavitation did not occur in the model. Cavitation in the prototype was due to a local condition which did not exist in the model and was not expected to exist in the prototype. At velocities of about 175 ft per sec, under conditions approaching those in the prototype tunnel, there was no measurable damage to the model test blocks. At velocities of about 150 ft per sec in the prototype there was no damage to the tunnel except immediately below the misalinement.

Mr. Abrams concludes, "* * * this spillway, having failed to withstand a discharge of 13,500 cu ft per sec, is entirely inadequate to discharge the 200,000 cu ft per sec for which it was designed." This statement is certainly a pessimistic forecast which is not substantiated by the facts. Model tests showed that the tunnel was indeed capable of discharging the design flood. Cavitation is the result of high-velocity flow and the maximum velocity for 200,000 cu ft per sec will not be greater, proportionately, than that for 13,500 cu ft per sec, since the height of fall for both discharges is approximately the same. Thus, the tunnel has already been subjected to velocities which approach the maximum, and the effects of high-velocity flow in the tunnel where no imperfections exist have already been determined. Also, with increased discharges the possibility of cavitation occurring in the elbow is reduced in two ways: (1) The resulting greater depth increases the pressure at the boundary; and

(2) the greater centrifugal force exerted by the greater mass of water in passing around the bend further increases the pressure at the boundary.

If the writer's paper serves no other purpose than to encourage a more wary attitude toward the performance of any hydraulic feature with respect to possible cavitation damage, he will feel that a worthy purpose has been served. It is to be hoped that in the future various individuals will feel encouraged by the candor of the Symposium authors to contribute their cavitation experiences, especially when they are novel or unexpected.

Acknowledgment is made to Messrs. Bradley, Ball, and Locher for contributing additional examples of cavitation erosion as experienced by the Bureau of Reclamation.

GEORGE H. HICKOX,[19] M. ASCE.[19a]—The response to the Symposium on cavitation experiences has been very gratifying. It is clear that the difficulties caused by cavitation have been rather widespread, and are to be expected wherever high-head structures are in operation.

The experiences reported in the incompleted conduits of Ross Dam (Seattle, Wash.) by Mr. Bradley and in the temporary elbows at Grand Coulee Dam (Washington) by Mr. Ball are good examples of the way in which cavitation can be prevented, both by raising the general pressure level and by surface treatment to correct local reductions of pressure. These experiences are similar to those cited in the case of the Norris Dam (Tennessee) sluices.

Mr. Ball's notes on the beginning of cavitation damage in the Nevada tunnel of Boulder Dam (Arizona-Nevada) support the supposition that cavitation was the cause of the extensive damage in the Arizona tunnel. His description of the progressive nature of cavitation damage, supported as it is by direct evidence, is especially valuable and shows how an otherwise insignificant irregularity of surface may become a menace to the safety of an entire structure. If a smooth continuous surface, such as a tunnel, sluice, or face of a dam, contains an irregularity, either depression or projection, of sufficient size, cavitation will occur whenever the velocity is high enough and the general pressure level is low enough. If, then, the cavitation results in damage, the resulting depression may be deep enough to cause still further damage; and from that point on the effects are progressive and cumulative. For this reason, the writer wishes to endorse, heartily, Professor Kindsvater's statement that structures such as baffle piers that are subject to cavitation damage must not be depended on to insure the safety of dams.

Mr. Blaisdell asks if the noise level is related to the cavitation damage. In the case of the Norris sluices, there appeared to be a relationship between the duration of cavitation and the damage sustained, but there are no data to indicate that the rate of damage is related to the noise level. However, if a higher noise level indicates an increased intensity of cavitation, it would seem reasonable to expect that it would also be accompanied by an increased rate of damage.

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[19a] Received December 19, 1946.

Mr. Ball's observation of flashes of light in the tailrace is of interest as such flashes have also been observed on spillway aprons during the operation of sluices discharging at high velocity (83). His hypothesis that these flashes are caused by differences in the refractive and reflective properties of the water due to compression waves is probably correct. This hypothesis has been advanced by Professor Kindsvater (84). A similar explanation for the appearance of rapidly moving light bands accompanying the eruption of Paricutin volcano has been given by O. H. Gish (85). The flashes are not necessarily the result of compression waves set up by cavitation within the structure. In the case of the sluices discharging on the apron at Cherokee and Hiwassee dams, the flashes appeared to originate in the boundary of the jet from the sluice. It seems probable that cavitation existed in the vortices set up at the sides of the jet. It is believed that the sluices themselves were free from cavitation as they had been constructed after extensive model tests; and measurements on the prototype structure disclosed no regions of low pressure. The existence of cavitation at the boundaries of a jet has been described by Mr. Locher in his discussion of the jet pump.

Professor McNown advances certain analytical methods of studying cavitation which undoubtedly have merit. It is suggested that such methods be used with considerable caution, however, since mathematical predictions based on the assumption of irrotational motion may be quite misleading. An example may be found in the case he cites in which cavitation actually occurred at a value of $\mathbf{K} = 1.8$, although the vapor pressure of the water was not reached until $\mathbf{K} = 0.7$.

Suggestions for further research are found in the various discussions. Professors Robertson and Van Driest suggest that water may be capable of supporting considerable tension while in motion, and that the formation of cavities may thereby be affected materially. Professor Robertson's assumption that the tension is a function of time is an interesting hypothesis but is subject to verification. Cavities that appear downstream from the point of lowest pressure may be caused by the formation of vortices in a region of expanding flow. This subject is certainly open to further investigation. Professor Van Driest's observations appear to indicate considerable tension in the liquid at times when no cavitation was observed. The existence of tension is possible, of course, as it is known to exist, and can be measured, in liquids at rest. His analysis does not show any definite relationship between tension and the occurrence of cavitation. It may be of interest to note, however, that, within the limits referred to by Professor McNown, most experimenters have found a good agreement between the occurrence of cavitation and the approach of boundary pressures to the vapor pressure of the liquid. It should be noted in this connection, as pointed out by Professor Kindsvater, that measurements of fluctuating pressure by a manometer are subject to considerable damping because of the relatively high frequency of the fluctuations and the large mass of the manometer fluid.

The bibliography included with the Symposium was not intended to be complete. A complete bibliography of papers on cavitation runs through many hundreds of titles.

Much investigation and research will need to be done before the mechanism of cavitation is understood. The same is true of the manner in which cavitation causes damage. The subcommittee sincerely hopes that discussers of the Symposium will be able to devote some effort to the solution of these problems. In the meantime, it is hoped that the Symposium has served to indicate the dangers that may exist when the possibility of cavitation is neglected.

Literature Cited.—

(78) "Model Studies of Spillways," *Bulletin VI-1*, Boulder Canyon Project Final Reports, Bureau of Reclamation, U. S. Dept. of the Interior, Denver, Colo., 1938, p. 173. (*a*) p. 185. (*b*) p. 163. (*c*) p. 9. (*d*) p. 180. (*e*) p. 170. (*f*) p. 177. (*g*) p. 169.

(83) "Performance of *TVA Structures Studied*," by George H. Hickox, *Civil Engineering*, October, 1945, p. 467.

(84) *Ibid.*, December, 1945, p. 565.

(85) *Ibid.*, April, 1946, p. 178.

Corrections for *Transactions:* In September, 1945, *Proceedings*, on page 1066, change the heading "Bibliography" to read "Literature Cited" to agree with page 999. See also December, 1945, *Proceedings*, page 1571.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

THE SAFETY OF STRUCTURES

Discussion

BY ALFRED M. FREUDENTHAL

ALFRED M. FREUDENTHAL,[45] ASSOC. M. ASCE.[45a]—Discussers of this paper have been handicapped by the brevity of the published version, which is a wartime, paper-saving condensation of a considerably longer manuscript, that has been filed for reference in the Engineering Societies Library in New York, N. Y.[24a] Part 2, dealing with " Analysis of Particular Influences" has been cut most severely, whereas not much condensation was possible in Part 1, the general and mathematical treatment. The result has been a slight distortion of perspective by the emphasis on the statistical and mathematical aspects of the problem rather than on the engineering aspects.

The impression appears to have been created that the writer had tried to put forward the claim that the theory of probability and statistical methods could be expected to provide the "perfect answer" to a problem as complex as that of the safety of structures. Actually, the purpose of the paper was to draw attention to the fact that, by the application of the theory of probability (probably the most efficient tool of modern scientific thought) the concept of safety can be rationalized, and to develop a method for the evaluation of the minimum safety factor on the basis of the available objective evidence instead of the usual procedure of arbitrary selection. Not only is it evident that the successful application of the method depends on the extent and reliability of the evidence, but that a certain "residue" of unknown and subjective factors will always be present, requiring appraisal on a level beyond that of statistical inference and prediction from the objective analysis of recorded past experience. That rationally unpredictable "residue" can be isolated by a rational analysis of all statistically predictable influences. Reliable data for this analysis, however, can only be obtained by following Mr. Hirschthal's call for "research and more research."

NOTE.—This paper by Alfred M. Freudenthal was published in October, 1945, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1946, by F. H. Frankland, and Elliott B. Roberts; February, 1946, by A. G. Pugsley, and Lynn Perry; April, 1946, by Nomer Gray; and June, 1946, by I. M. Nelidov, and M. Hirschthal.

[45] Lecturer in Bridge Eng., Hebrew Inst. of Tech., Haifa, Palestine.

[45a] Received November 7, 1946.

[24a] 29 West 39th St., New York 18, N. Y.

In his thoughtful comments, Mr. Frankland has raised the important point of the effect on the safety of structures of the distribution of elastic stresses through inelastic behavior. There is no doubt that "some of the generally accepted underlying concepts of structural design are inadequate." It is really paradoxical that one's ability to analyze structures should depend on the assumption that structures are perfectly elastic, whereas the only real safeguard insuring satisfactory performance in service is the extent to which structures are inelastic. This inelastic behavior, by producing a certain redistribution of stresses, necessarily affects the safety. However, by devising an appropriate "mechanism of resistance" and establishing its range of uncertainty, that effect can easily be considered in the evaluation of the safety factor.

It is too frequently forgotten, however, that the resistance mechanism essentially depends on the rate of application of stress. This is shown by the variation of the stress-deformation diagrams of concrete (Fig. 10(*a*)) and of steel

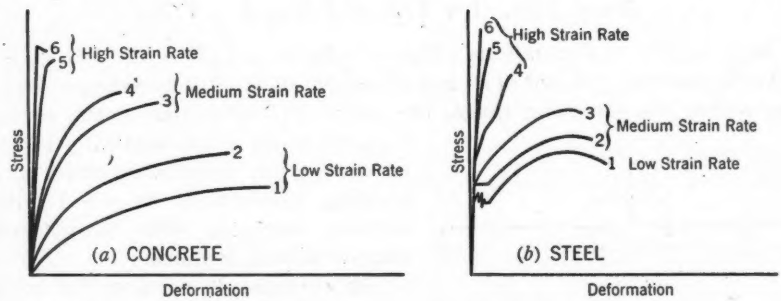

Fig. 10.—Influence of the Rate of Deformation on Deformability and Strength

(Fig. 10(*b*)). The structural member or connection in a steel frame which accommodates itself to slowly applied overstress by plastic redistribution of elastic peak stresses may fail without redistribution by cracking in a brittle manner if the same stresses are applied rapidly. This fact alone would contradict Mr. Frankland's assumption that "* * * the theory of limit design will explain the capacity of a structural material to sustain occasional overstrain without damage to the ultimate endurance limit * * *." This theory which, under the name of "Classical Theory of Plasticity"[46] has been studied very extensively in prewar Europe since the late 1920's, is certainly not applicable to conditions of repeated, rapidly moving loads which are implied in the term "endurance limit"; it has been applied, in general, to structures subjected to steady or to slowly moving loads only. However, even for this type of load values of the resistance or the carrying capacity are obtained which might be considered theoretical extremes never attainable in practice. The real values should be somewhere between that extreme and the minimum value, defined by the state of elastic stress immediately preceding the occurrence of the first plastic strain in the most highly stressed section. Such a concept is borne out

[46] *Preliminary and Final Reports*, 2d Cong. of the International Assn. for Bridge and Structural Eng., Berlin, 1936, question I.

by the results of experimental investigations between 1930 and 1936[47] which have led to the replacement of the rather crude "classical" concept of plastic redistribution of stress by the so-called "New Theory of Plasticity."[48]

According to the classical theory of plasticity, the upper resistance limit of a steel section in simple bending is defined by a fully plastic rectangular stress distribution. The lower limit is reached when the extreme fiber stress of the triangular (elastic) stress distribution attains the yield limit s_y. Since the scarcity of the available experimental evidence does not justify a statistical interpretation of the results, this is an example of the midpoint value between known extremes, being the most reasonable tentative assumption for the design value; and this value is subject to fluctuations within the range delimited by the extremes. Let S denote the elastic section modulus; T, the plastic section modulus (which is the sum of the static moments of the section elements about the neutral axis); the design value of the resisting moment can be expressed as:

$$\bar{M}_R = \tfrac{1}{2} S \, s_y \, (1 + T/S) = \tfrac{1}{2} \bar{M}_{RO} \, (1 + T/S) \qquad (34)$$

Eq. 34 is subject to fluctuations within a range of $\pm \, [(T - S)/(T + S)]$.

As the resisting moment of an individual section is subject to chance fluctuations within the foregoing range, the range of fluctuations of the carrying capacity of an n-fold statically indeterminate girder, being a function of the resisting moments of its $n + 1$ critical sections, increases with an increasing number of such sections.

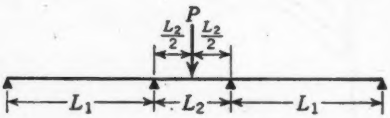

FIG. 11

As an example, consider the continuous beam shown in Fig. 11 under the loading conditions indicated. The capacity load, P_{El}, under the conventional assumptions of the elastic theory can be expressed by

$$P_{El} = \frac{4 \, S_m \, s_y}{L_2 \, (1 - \alpha)} \qquad (35)$$

in which S_m denotes the section modulus at the point of load application and

$$\alpha = \frac{3 \, L_2}{4 \, L_1 + 6 \, L_2} \qquad (36)$$

For a freely supported central span, $L_1 = \infty$ and $\alpha = 0$ and the capacity load,

$$P_o = \frac{4 \, S_m \, s_y}{L_2} \qquad (37)$$

The ratio—

$$\frac{P_{El}}{P_o} = \frac{1}{1 - \alpha} \qquad (38)$$

—varies between 1.0 for the freely supported central span and 2.0 for a central span with perfectly fixed ends defined by $L_1 = 0$ and $\alpha = 0.5$.

[47] "Test Results, Their Interpretation and Application," by H. Meier-Leibnitz, *Preliminary Report*, 2d Cong. of the International Assn. for Bridge and Structural Eng., Berlin, 1936, pp. 103–137.

[48] "Fundamental Principles of the Theory of Plasticity," by I. Fritsche, *ibid.*, pp. 15–41; and "Suggestions," question I, pp. 933–934.

According to the theory of limit design, the capacity load, P_{LD}, of the structure is reached with the formation of three (that is, $n + 1$) plastic hinges over the supports and at the midspan of the central span. Therefore, it is independent of α and can be expressed by

$$P_{LD} = \frac{4\,(T_m + T_s)\,s_y}{L_2} \qquad (39)$$

in which T_m and T_s denote the plastic section modulus at the midspan of the central span and over the supports, respectively. For a girder with constant sections, $T_m = T_s = T$:

$$P_{LD} = \frac{8\,s_y\,T}{L_2} \qquad (40a)$$

and

$$\frac{P_{LD}}{P_o} = \frac{2\,T}{S} \qquad (40b)$$

Under the assumption that the most probable value $\bar{P}$ of the capacity load is the mean of the elastic and the limit-design values:

$$\bar{P} = \frac{2\,\bar{M}_R}{L_2} \times \frac{3 - 2\,\alpha}{1 - \alpha} \qquad (41a)$$

and

$$\frac{\bar{P}}{P_o} = \frac{1}{4}\left(1 + \frac{T}{S}\right)\frac{3 - 2\,\alpha}{1 - \alpha} \qquad (41b)$$

The mechanism of resistance of the girder defined by the "design" load $\bar{P}$ is subject to fluctuations between the extremes within a specific range of $\pm \frac{1 - 2\,\alpha}{3 - 2\,\alpha}$, as well as to fluctuations due to the uncertainty in the individual resistance mechanism of every one of the three critical sections. Hence, the capacity load is:

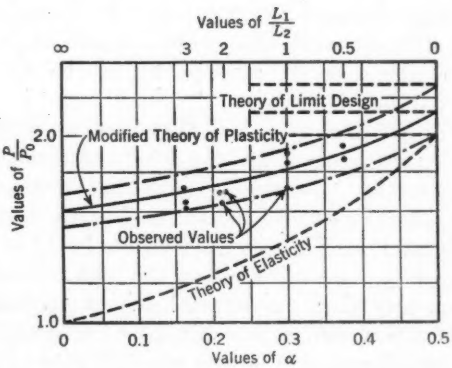

FIG. 12

$$P = \bar{P}\left[1 \pm \sqrt{\left(\frac{1 - 2\,\alpha}{3 - 2\,\alpha}\right)^2 + 3\left(\frac{T - S}{T + S}\right)^2}\,\right] \qquad (42)$$

Comparison of Eq. 42 with the results of tests performed by F. Stuessi and G. F. Kollbrunner[49] and H. Meier-Leibnitz[50] has been presented in Fig. 12; the experimental values are scattered within a comparatively narrow range about the expected or "design" value $\bar{P}$ computed with the aid of Eq. 41*a*, the actual range of variation being much narrower than the range between the extremes as expressed by Eq. 42.

[49] "Beitrag zum Traglastverfahren," by F. Stuessi and G. F. Kollbrunner, ***Bautechnik***, Berlin, 1935, Vol. 13, p. 264.

[50] "Relations in Girders Continuous Over Three Spans," by H. Meier-Leibnitz, ***Final Report***, 2d Cong. of the International Assn. for Bridge and Structural Eng., Berlin, 1936, p. 70.

The foregoing example illustrates that a design based on the assumption of perfect elasticity is frequently inadequate; it also shows that a design based on a somewhat crude concept of plastic behavior, as expressed by the classical theory of limit design, may be less adequate. However, the real, rather complex behavior can be fairly well approximated by a simple assumption. This serves to support the writer's assertion (see heading, "Part II. Analysis of Particular Influences: Uncertainty and Inaccuracy of the Mechanism of Resistance") that

> "* * * it is hardly possible to conceive such a mechanism [of resistance] that will effectively reproduce the actual phenomenon and which is at the same time simple enough to be suitable for practical design. Every devised mechanism, therefore, is fictitious to a certain degree * * * Its suitability is to be judged by the simplicity of the concept, by the closeness with which experimental results are reproduced, and by the narrowness of the range of dispersion of such results about the 'theoretical' course."

The writer regrets (certainly not less than Lt.-Comdr. Elliott B. Roberts) the lack of a discussion of earthquake factors, and he agrees fully with the assertion that although "Evaluation of the [earthquake] risk in a specific location may introduce a delicate problem; it should never be ignored completely." A short comment on the principal aspects of this problem is included in the complete manuscript.[24a]

Although it is true that "no one could safely state that any place is perfectly free from the danger of destructive earthquakes," the frequency of occurrence of seismic forces at one location is hardly of an order of magnitude comparable to that of the occurrence of primary design loads (service loads or impact) and of secondary effects (wind or temperature changes). Even in the principal seismic areas of the world, earthquake shocks occur rather less frequently than maximum loads or extreme wind velocities. Therefore, their effects should reasonably be considered on the same level as those extreme conditions—that is, as a source of stress fluctuation of appropriate range about the stresses produced in the structure by the design values of primary loads and secondary effects. Because of the relative infrequency of earthquakes of any intensity there is hardly any justification for introducing even a small seismic acceleration force as "design value." There is even less justification for considering the stresses produced in the structure by acceleration forces which result from severe shocks (from 0.10 g to 0.15 g) similar in character to the stresses produced by the design service loads, and to compare them with the conventional values of permissible stresses.

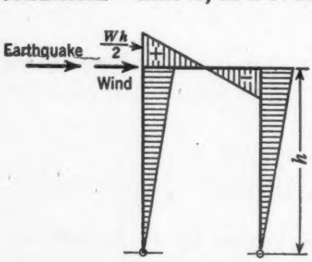

Fig. 13

A procedure recommended for use in the design for seismic forces can be illustrated by reference to the portal frame shown in Fig. 13, which represents the pier of a viaduct. Both the wind forces on the structure and the seismic acceleration force acting at its center of gravity are transmitted into the frame

at its top corner. If it is assumed that (*a*) the extreme wind force P_w exceeds the "design" force W_o by 100%, (*b*) the maximum seismic force P_E is of the order of magnitude of the design wind force, and (*c*) the maximum range of fluctuations in the resistance value s_r is $\Delta s_r = 0.2\, s_{ro}$, then the corners of the portal bracing strut should be proportioned for the bending moment $M = \frac{1}{2} W_o h$ with a safety factor (see Eqs. 5 and 18):

$$\zeta = \frac{1 + \sqrt{\left(\frac{\Delta W}{W_o}\right)^2 + \left(\frac{P_E}{W_o}\right)^2}}{1 - \frac{\Delta s_r}{s_{ro}}} = \frac{1 + \sqrt{1 + 1}}{0.8} \cong 3 \quad\text{.......(43)}$$

Eq. 43 provides for the stresses occasionally produced by seismic forces of the assumed magnitude. If earthquake effects were not considered in the design, the strut would be proportioned for the same bending moment but with a safety factor:

$$\zeta = \frac{1 + \frac{\Delta W}{W_o}}{1 - \frac{\Delta s_r}{s_{ro}}} \quad\text{.......................(44)}$$

From which $\zeta = \frac{1 + 1}{0.8} = 2.5$. For the strut under consideration a 17% reduction in the permissible stress, therefore, would be sufficient to provide for the effects of an earthquake force of the magnitude of the design-wind force. In this example, the absence of vertical loads tends to intensify the effect of the seismic forces.

The conventional procedure of earthquake design—in which the effects of horizontal acceleration forces ($= 0.1\, g$) are simply added to those of vertical loads and of secondary influences—undoubtedly provides an excessive safety reserve. Nevertheless, the acceleration to be used in the proposed procedure may possibly have to exceed $0.10\, g$ since it should reproduce extreme earthquake effects. Only very careful and extensive statistically interpretable observations, and analysis of seismic vibrations both of the ground and of structures, can provide a reply to this last question.

Designers of aircraft structures operate with the aid of a considerably more advanced concept of safety than is generally applied in civil engineering. This fact is shown clearly by Professor Pugsley's stimulating comments; and it is not surprising, since in aircraft design any reduction in the weight of structural material which may result from an advanced approach to the problem of safety will improve the performance of the aircraft and effect a direct saving in the initial cost.

Efficiency and economy are of such immediate concern to the air transport industry as to justify unusually large appropriations for research, particularly in wartime. The principal object of such research must be to evaluate the cumulative fatigue effects of repeated stress cycles of varying amplitude, in relation to the capacity of the material to sustain "damage by overstrain" (see heading, "Part 2. Analysis of Particular Influences: Uncertainty and

Variation of Resistance Limits of Structural Materials"). In a number of excellent papers Professor Pugsley[51,52] and others have analyzed this difficult problem thoroughly and have presented important results. It appears, however, that even in the aeronautical field the results from experiment and observation are rather erratic, the correlation between the principal factors being far from satisfactory. What is probably needed is not only research on a large scale, but an entirely new approach to the problem of fatigue. The writer has attempted to provide such an approach[53] by establishing the statistical character of fatigue as an expression, on a macroscopic scale, of the progressive destruction of cohesive bonds. Fatigue is the result of the repetitive action of an external load and has the typical features of a mass phenomenon, both the cohesive bonds and the load repetitions being collectives in a statistical sense. By combining the probabilities of bond destruction for various amplitudes and numbers of stress cycles the cumulative damage effect can be evaluated and predicted.

The distinction between "absolute" and "economic" safety admittedly presents a very delicate problem; and it is evident that the approach to this problem will be affected by the character of the structure and the seriousness of the consequences of failure. For military aircraft Professor Pugsley's recommendation that strength factors should be chosen so that "structural accident rates shall be as small as possible consistent with efficient production and operation" is probably adequate. Civil engineering structures, on the other hand, must be designed sufficiently safe to make the chance of one single occurrence of a critical or of an undesirable condition negligible. It should be recognized, however, that "absolute" safety is unattainable and that the admission of a finite (however small) probability of failure is inherent in the statistical approach.

The writer very much regrets that by using the word "strain" in a sense which differs from the established meaning of "specific deformation" the intelligibility of the paper should have suffered—a remark made by both Mr. Perry and Mr. Gray. In favor of such use the writer would like to point out that, in contrast with other languages (French or German), the English technical language lacks a term expressing the general effect produced in a structure by loads and other influences (French: *Effort and sollicitation;* German: *Beanspruchung*). The common use of the word "stress" in this general sense is still more confusing than the writer's use of "strain"; "stress" is an abstract concept implying the action of a mechanical force per unit area, whereas "strain," even in the established meaning, is the observable effect on the structure, or part of it, of influences which are not necessarily forces. Direct correlation between strain and failure has been established reliably. Although fracture does not occur unless some strain is present, the action of a force is not essential. Furthermore, the meaning of the word "strain" in nontechnical language represents, very well, the meaning proposed by the writer.

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[51] "Modern Experimental Work on Aeroplane Structures," by A. G. Pugsley, *Journal*, Royal Aeronautical Soc., London, September, 1945.

[52] "Specification of Loading Conditions for Strength Tests on Aeroplane Structures," by A. G. Pugsley, *Aircraft Engineering*, London, December, 1945.

[53] "The Statistical Aspect of Fatigue," by A. M. Freudenthal, *Proceedings*, Royal Soc. of London (publication pending).

Mr. Perry's comments on the fundamental difference between the economic aspect of structural safety prevailing in the United States and in prewar Europe are very much to the point. This difference is the result not only of the substantial difference in the ratio of the cost of material and labor but also of the relative importance of standardization of production and organization of work in a country the size of the United States. The implication is unjustified, however, that because of the comparatively low cost of materials there is no need to rationalize the fundamentals of design and that the "experienced designing engineer" can be relied upon to provide all the answers by pure intuition. Such an approach to engineering denies the value of research as an instrument of progress and leads logically to "trial and error" as the only method of making engineering progress, and "apprenticeship" as the only appropriate system of engineering education.

Although it may be perfectly obvious that "money can be saved by decreasing the load or by increasing the allowable unit stress" this statement contributes nothing toward the rational appraisal of the effect of load reduction or of a unit stress increase on the real value of the safety factor. Nine out of ten "experienced designing engineers" will not be able correctly to evaluate this effect; nor will they be able to justify, by rational argument, a definite numerical value for the safety factor. The engineer's client is generally the least qualified person to specify the loading and the allowable stresses. To specify the actual loads to be carried by the structure at present and expected in the future is the best he can be expected to do; but he generally lacks the knowledge necessary to translate these data into a workable loading specification and to select the rational safety factor pertaining to this specification.

Failures of well-designed structures have almost never been caused by simple overloading except in cases of gross neglect. They have mostly resulted from too much reliance on the past experience of the designer, and on his failure to recognize the emergence of new factors in the design.[54]

The crash of a truck into an end post of a bridge, or of a ship's mast into a truss chord of a bridge, should be prevented by the specification of adequate width of curbs and of ample clearances; it has nothing to do with the factor of structural safety, not even in "academic discussion."

Although it should provide an adequate leeway for the guidance of a very careful designer, the writer's proposal to provide for a 10% fluctuation of dead load is tentative, and does not appear to be as ample as Mr. Perry assumes. It is significant in this connection that the dead load of the original Quebec Bridge, computed after its collapse, exceeded the design load by some 20% to 30%.[55] An investigation conducted during the erection of a 48-story building[56] on eight of its main columns furnished the following ratios between the observed dead load stresses and those computed on the basis of the completed drawings: 1.20, 1.31, 1.23, 1.29, 1.27, 1.26, 1.22, and 1.30—an average of 1.26.

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[54] "Dynamics and Bridge Design," by A. M. Freudenthal, *Engineering News-Record*, Vol. 129, 1942, pp. 540–541.

[55] "Structural Engineering," by G. F. Swain, McGraw-Hill Book Co., Inc., New York, N. Y., 1st Ed., 1927.

[56] "Dead Load Stresses in the Columns of Tall Buildings," *Bulletin No. 40*, Eng. Experimental Station, Ohio State Univ., Columbus, Ohio.

The fact that structures designed for a wind load of 30 lb per sq ft have given satisfactory service is hardly surprising, since such a wind load represents the pressure exerted by a storm of the infrequent velocity of about 85 miles per hr. This value is derived from the known formula,

$$p = \frac{c\, q\, v^2}{2\, g} = 0.00256\, c\, V^2 \ldots\ldots\ldots\ldots\ldots\ldots (45)$$

in which V denotes the wind velocity in miles per hour and c is an empirical factor of shape, which for structural shapes varies between 1.3 and 1.9.[57]

Wind velocities fluctuate within a wide range; wind records taken at a number of stations at New York, N. Y.,[58] reporting maximum velocities and covering several years, show that not more than one observation out of every ten recorded velocities in excess of 46 miles per hr at about 500 ft above ground level and 65 miles per hr at 1,250 ft above ground level. The maximum velocities at these heights were 71 miles per hr and 102 miles per hr, respectively. A design load of 20 lb per sq ft corresponding to a wind velocity of 60 miles per hr should be more than adequate under normal climatic conditions. An occasional increase to as much as 40 lb per sq ft (V = 100 miles per hr) would have to be covered by the safety factor. Such reduction of the design wind load would not controvert the fact that a number of serious accidents have occurred as a result of wind action (Tacoma-Narrows Bridge in the State of Washington, and Chester Bridge in Chester, Ill.). These accidents were not caused by excessive wind load but by the failure to provide adequately for the accompanying dynamic effects, and for uplift.

The lack of uniformity in the properties of structural materials is an important source of fluctuation of structural resistance. For steel, the range of fluctuation inherent in the individual manufacturing process is further increased in the structure by the fact that structural shapes in one framework may originate not only from the top or the bottom of ingots of the same heat or from ingots of different heats in the same manufacturing process, but from entirely different processes and mills. Moreover, the properties are affected by the rolling, and they differ for different shapes and thicknesses. The frequency distributions drawn in Fig. 3 represent the yield limit and tensile strength of an alloy steel in which all those sources of nonhomogeneity have been present. Nevertheless, the fluctuations are enclosed within a range of from 15% to 20% above and below the mean value. Even for concrete produced on the site, the manufacturing process of which is considerably less controlled than that of steel, the range of fluctuation of the compressive strength about its modal value seldom exceeds ± 35%. This is shown in Fig. 14 which represents the results of an investigation involving more than 1,000 samples.[59] It is interesting to compare the foregoing value (± 35%) with the range of fluctuation of the compressive strength of concrete manufactured under laboratory conditions which, normally, does not exceed ± 15%.

[57] "Wind Pressure on Structures," by G. E. Howe, *Civil Engineering*, March, 1940, pp. 149–152.

[58] "Wind Forces on a Tall Building," by J. Charles Rathbun, *Transactions*, ASCE, Vol. 105, 1940, pp. 8–11.

[59] *Ibid.*, Vol. 96, 1932, pp. 1367–1371.

The writer heartily endorses Mr. Gray's comments.[59a] The lack of interest in fundamentals among engineers is really surprising. The number of discussers of a paper concerned with methods of elastic stress calculation, for instance, is generally a multiple of those taking part in the discussion of paper concerned with basic principles.

With regard to the safety of main cables of suspension bridges there can be no doubt that here is a structural element which probably requires an exceptionally low safety factor. If the frequency distribution of the strength of individual wires is narrow, that of the cable is still narrower, since the standard deviation of the strength of a cable consisting of m wires would be $\frac{\sigma}{\sqrt{m}}$ if σ denotes the standard deviation for the individual wire.

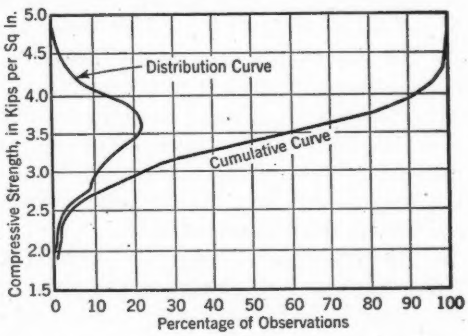

FIG. 14.—FREQUENCY DISTRIBUTION OF THE CYLINDER STRENGTH OF CONCRETE (MODAL STRENGTH APPROXIMATELY 3,600 LB PER SQ IN.)

As correctly stated by Mr. Gray, the uncertainty regarding the loading is also exceptionally small. However, the writer is not so sure about the reliability of the computed stress, particularly with regard to the uniformity of the distribution of stress over the individual wires. Since the capacity of the hard wire to secure inelastic relief of local overstress is rather small, the initial uniformity of stress distribution appears to be the principal question that needs careful study before designers can undertake the probably justified substantial reduction of the safety factor for suspension bridge cables.

Mr. Nelidov comments on the difference in the treatment of homogeneous and nonhomogeneous states of stress. This is a rather complex problem which, in most engineering applications, is best solved by the introduction of an adequate "mechanism of resistance"; this mechanism is an empiric function of the geometrical dimensions and the observable elementary physical properties. The variability of the resistance which determines the safety factor can be either directly evaluated from test results or computed by applying Eqs. 16 or 18 to the results of tests concerned with the observation of the constituent properties.

In a more scientific approach to this problem the probability of failure at every point of the stress field could be expressed as a function of the coordinates, considering both the variable stress intensity and the frequency distribution of the strength of an element. This approach leads to rather involved statistical computations; but it must be applied if no reasonable "resistance mechanism" can be devised a priori.

The essential difference between both approaches can be illustrated by considering the case of simple bending of a section composed of wires. The "re-

[59a] The remarks of both Mr. Perry and Mr. Gray concerning Eqs. 6*a* and 6*b* result from a regrettable misprint on the left side of the equations which should read s_e and s_r, respectively, instead of ζ (see June, 1946, *Proceedings*, p. 888).

sistance mechanism" of the section expresses the resisting moment in terms of the geometrical dimensions and the tensile strength of the material, the assumption being that failure is always initiated in the extreme fiber of the section. In contrast to this assumption, in the strict statistical approach, a definite probability of failure in relation to the distance from the neutral axis and to the fluctuation of strength is attributed to every wire of the section. Fracture may thus start in any individual wire, whatever its stress, if its actual strength is low enough. The resulting frequency distribution of sectional resistance is then used to evaluate the safety factor of the entire section.

The importance of a breadth of vision in predicting future developments, particularly with regard to the weight of railroad traffic is rightly stressed by Mr. Hirschthal. There is no other type of structure for the economic design of which the adequate appraisal of such development is as essential as for the railroad bridge. Here this aspect tends to overshadow all other aspects of the design; therefore, rational analysis and evaluation of the initial safety factor loses much of its practical value.

The difficulties of design for repeated stress cycles, to which Mr. Hirschthal refers, have been dealt with in the paper and the available quantitative results presented in Figs. 6 and 7. Admittedly, these results do not as yet fully account even for the most elementary conditions encountered in the design of structures subject to dynamic loads.

The writer greatly appreciates the stimulating and constructive comments of those who have discussed the paper and wishes to convey to them his thanks for their contributions.

Corrections for *Transactions*: In October, 1945 *Proceedings*, page 1162, in Eq. 6*a*, change "ζ" to "s_a" and in Eq. 6*b*, change "ζ" to "s_r"; in Eq. 17 change "∂X_n" to "∂x_n"; and in Eq. 32 change "s_{ao}" to "s_{fo}" in two places and "s_a" to "s_f" in one place. In February, 1946, *Proceedings*, on page 253, change the sentence beginning with line 32 to read: "With Eqs. 6*a* and 6*b*, the author computes the maximum strain and the lowest value of the structure's resistance using equations containing arbitrary constants. In principle, this is no different from current practice." In April, 1946, *Proceedings*, on page 560, delete the two sentences in lines 28 to 31, inclusive. Other errata were published in June, 1946, *Proceedings*, page 888; in April, 1946, *Proceedings*, page 560; and in October, 1945, *Proceedings*, page 1187.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

TORSION IN STEEL SPANDREL GIRDERS

Discussion

BY J. E. LOTHERS

J. E. LOTHERS,[22] M. ASCE.[22a]—The able discussion of this paper has been no mean contribution to structural engineering literature and its courteous flavor has been most flattering and gratifying. The paper itself was an attempt to present a more or less simple method for computing torsional shear in steel spandrel girders. The discussion was a masterful job of providing details and embellishments, calling attention to shortcomings and exceptions, and indicating the mitigating effects of fireproofing and other factors encountered in practice. In his closing discussion the writer can do little else, aside from expressing his appreciation to these accomplished writers, than to agree with most of the hypotheses.

Professor Fiesenheiser is correct, of course, in stating that a standard riveted connection would not suffice at joint B, Figs. 7 and 8. He gives a very complete demonstration of the application of Prof. J. Charles Rathbun's method of correcting for connection restraint to torsional analysis. In his introductory remarks Professor Fiesenheiser mentions that the ratio Z of the angle of rotation θ_M of the end connection to the moment M producing it is determined by test. The constant Z may be computed and the writer hopes to have equations for that purpose published. It can be demonstrated, for example, that the small connection mentioned by Professor Fiesenheiser may be subjected to a moment of 5,780 in.-lb without exceeding the allowable bending stress of 20,000 lb per sq in. in the outstanding legs. This condition would seem to refute Professor Gant's contention that a standard riveted connection would not develop the torsion in a spandrel girder.

Eq. 17 (Professor Gant) for correcting for the effects of deflection and Eqs. 18 to 29 (Professor Hoffman), inclusive, illustrating the application of the calculus of finite differences to the torsion problem are gratefully acknowledged.

NOTE.—This paper by J. E. Lothers was published in March, 1946, *Proceedings*. Discussion of this paper has appeared in *Proceedings*, as follows: June, 1946, by John E. Goldberg, and I. Oesterblom; October, 1946, by Robert V. Hauer; and November, 1946, by E. I. Fiesenheiser, Edward V. Gant, Oscar Hoffman, and Phil M. Ferguson.

[22] Prof. of Architecture, School of Architecture, Oklahoma Agri. and Mech. College, Stillwater, Okla.

[22a] Received December 23, 1946.

Professor Ferguson calls attention to the modifying effects of the enveloping concrete slab and fireproofing. Since a steel beam without fireproofing is rarely found in important steel construction, his discussion is very pertinent. The writer "laid himself open" when he specified concrete fireproofing in Example 3. In so doing, however, he was endeavoring to arrive at practical and readily computed loads, and the stiffening effects of the resulting composite beams and girders were neglected for the purpose of simplicity in demonstrating the proposed method of solution.

Professor Ferguson also called attention to the fact that Eq. 12*b* is not mathematically correct and stated that the practical difference is not important. Eq. 30 can be further simplified to:

$$M_{AB} = \frac{6\,S_T}{4\,S_T + 3\,S_F} \times (\text{FEM})_{AB} \ldots\ldots\ldots\ldots\ldots\ldots (31)$$

The derivation of Eq. 31 was demonstrated in the oral presentation of the paper before the Oklahoma Section at Tulsa on April 28, 1945.[23] Undoubtedly, the derivation (indicated in Professor Ferguson's discussion) should have been included in the published version. It will be noted, however, that Eq. 12*c* was given for a condition of end fixity that was the average of the conditions covered by Eqs. 12*a* and 12*b*. To have averaged Eqs. 12 and 31 would have led to an awkward form for Eq. 12*c* that would not have been justified in view of the rather rough assumptions of end fixity for the three cases covered by Eqs. 12. As Professor Ferguson has stated, S_T is very small as compared with S_F; and, accordingly, the coefficient of S_T in the denominator of Eq. 31 may be changed from 4 to 3 without affecting slide rule computations. Eq. 12*b* results if this substitution is made.

Corrections for *Transactions:* Errata were published in October, 1946, *Proceedings*, on page 1200.

[23] *Civil Engineering*, June, 1945, p. 295.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

NEW PROJECT FOR STABILIZING AND DEEPENING LOWER MISSISSIPPI RIVER

Discussion

BY CHARLES SENOUR

CHARLES SENOUR,[11] M. ASCE.[11a]—The writer is grateful for the several discussions of his paper and for the consideration and thoughtfulness which they exemplify.

With regard to Mr. Okey's arresting reference to a paper by the late Maj. T. G. Dabney (to whose memory a handsome granite marker has been erected at the head of the Yazoo Basin levee), it may be stated that, although this particular paper has not come to light either at Vicksburg (Miss.) or Clarksdale (Miss.), the desirability and eventual necessity for more or less complete stabilization of the banks of the lower Mississippi have been voiced by various writings of Major Dabney both prior and subsequent to 1911, and also, from time to time, by others. The very considerable sum of money and the large plant investment involved in such an undertaking, the necessity of accomplishing the work at a fairly rapid tempo, and the apparently more urgent need for other types of work have, in the past, inhibited anything beyond academic consideration of such a project. Authorization of the present undertaking was made possible largely through the efforts of Maj.-Gen. Max C. Tyler, M. ASCE, who was president of the Mississippi River Commission between 1939 and 1945.

With reference to the question of the direct effect of bank stabilization upon actual depths over the crossing bars without low dikes to direct low-water flow, it may be remarked that Mr. Okey's observations are borne out by the model studies made at the Waterways Experiment Station at Vicksburg. In all cases the model river bed lowered when completely stabilized. In some cases, the resultant actual low-water depths over the crossing bars increased; in others the bed and water surface were lowered in practically equal measure. It seems certain that at many points dredging must continue even after sta-

NOTE.—This paper by Charles Senour was published in February, 1946, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: September, 1946, by Charles W. Okey, and F. Newhouse; and November, 1946, by Gerard H. Matthes.

[11] Chf. Engr., Mississippi River Comm., Vicksburg, Miss.

[11a] Received December 13, 1946.

bilization, but, it is hoped, with greatly improved results in navigable depths for equal effort.

As Mr. Newhouse states in the preamble to his interesting analysis, the necessity for bank stabilization can be deduced from basic theory. It is not believed that—for many years past, at least—engineers have cherished the hope that the construction of levees could solve the meander problem. Mr. Newhouse mentions that the shape of a river channel depends upon two factors only—the slope, and the character of the soil through which the river flows—and that slope depends only upon the fall from source to mouth and the length of the stream's course between those points. So inextricably interrelated are the factors that govern river regimen, that, although the shape of the channel does indeed depend upon slope and soil characteristics of bed and banks, in a large degree, the slope, itself (at least with respect to quite long reaches of the stream), is determined by the soil characteristics of bed and banks, tough materials apparently conducing to flat, and sandy materials to steeper, slopes.

Mr. Newhouse also stresses a very important point in his comment as to the extent of the cross section revetted upon the Mississippi River, and the possibilities of trouble inherent in revetment of the banks alone. It is true that, with only the banks protected, the river bed will generally scour at the outer edge of the revetment. On the Mississippi River the revetment is normally built to the toe of the bank's slope and beyond the toe a distance sufficient to permit it to follow down as deepening occurs. The assumption is made, for purposes of design, that the ultimate depth attained will approximate the maximum depth observed elsewhere in the general vicinity where bank recession is not in progress. The revetment is articulated and so is flexible; it has considerable strength in tension. By virtue of these characteristics it is able, in some cases, to adjust itself to a moderate amount of gradual deepening without apparent distress. In other cases, repairs are required. The process of adjustment takes place a great distance beneath the surface of the muddy stream and its mechanics are not observable. It seems probable that, when the adjustment takes place by a gradual flowing out of sand from beneath the mattress, the latter accommodates itself to the change without much damage. However, when the character of the bank is such that a considerable deepening occurs in advance of bank movement, and for this or some other reason the movement partakes of the character of a slough of sizable proportions, the outer parts of the mattress, despite its considerable strength, are ruptured and replacement of the riverward sections is indicated. Plans have been under consideration for some time for a large-scale experiment with clear water to continue to investigate the mechanics of failure, but the problem poses certain practical difficulties. The matter is also being studied from another angle—the possibility of using low abatis dikes or similar permeable current-retarding structures sunk and anchored at the toe of the revetment to prevent erosion at that immediate point and to force the locus of deepening farther channelward. Some experimental work along this line has been performed on model rivers, and it is possible that an installation will be made in the prototype during 1947.

Mr. Matthes' scholarly observations anent the contrasting characteristics of the Mississippi and Mesopotamian alluvial valleys are most interesting, as is his discussion of the implications and limitations of the term "poised" as used to connote the river's lack of apparent tendency to aggrade or degrade. As to aggradation of the flood plain, the deposition of silt from overbank flows took place for centuries along the present meander belt of the river before its floods were confined by levees which, in a geological sense, are parvenu indeed. The heavy deposits, however, consisting of the coarser sediments, took place on the immediate banks, which, accordingly, in course of time, became natural levees sloping off landward into the lowlands upon which the silt deposits were less generous. Since the river's meander was constantly caving its immediate banks, it was constantly tending to work its way into lower and lower ground. Thus, while through the medium of its flood deposits it was always tending to increase the general elevation of its banks, it was at the same time tending to decrease bank elevation by caving the highest ground into the river, thereby forcing the bank line back into lower terrain—which in turn built up and in turn caved in, and so ad infinitum. The net result of all this "give and take" appears to be a present natural levee at about the same height as those of previous meander belts. It can scarcely be doubted that the general elevation of the lowlands too remote from the stream to be attacked by it must have increased through the years, but, as stated by Mr. Matthes, the deposits, except at the immediate banks, appear to have been comparatively light and—over most of the territory—have been stopped entirely by the construction of levees. Some building of the batture (the land lying between the levee and the river) is in progress, particularly below Baton Rouge, La., where in many places the levee looks appreciably higher when viewed from the land side than it does when viewed from the river side. Hydrographic surveys, repeated at intervals over most of the period of levee building, have revealed no general tendency toward any aggradation of river bed. The necessity for upward revision of levee grades from time to time has been brought about entirely by the increasingly more complete confinement of flood flows warranted by the continuing economic development of the alluvial plain.

W. E. Elam, M. ASCE, chief engineer, Board of Mississippi Levee Commissioners, has advised by letter that the assertion that none of the levees of the lower Yazoo Levee District remains upon its original alinement is in error—that according to a map of the district made in 1867 by its then chief engineer, Minor Meriwether, he has located (in small stretches scattered here and there through the almost 180 miles of line) a little more than 27 miles that the river has overlooked. The assertion referred to has been current for some years in this vicinity. It apparently represents an erroneous conclusion drawn from a statement in the 1943 report of the levee board to the effect that the river had claimed 305 miles of its levees since 1880. The writer is at a loss to explain such crass inefficiency in a stream as generally thorough in such matters as is the Lower Mississippi, but is glad to make the correction.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

SOME THOUGHTS ON ENGINEERING EDUCATION

Discussion

By C. A. Dykstra, and Louis Balog

C. A. Dykstra,[36] Esq.[36a]—During the past few years, the writer has given some thought to engineering education, and has discussed the problem with many engineers and with members of engineering faculties. He agrees with the author that engineering training should be broad and fundamental and that it should place some emphasis on what is being called "general education." An engineer well grounded in mathematics and the necessary sciences who has some knowledge of the background of civilization, a reasonable mastery of the English language both spoken and written, and a speaking acquaintance with the fields of psychology and economics will have a better chance to advance in his profession than the one who too early acquaints himself with techniques and specialities in the field of engineering. These specialities can be mastered if and when the need for them arises. The mastery of a speciality will come with experience on the job, through special courses given by industry, and through the opportunities for advanced work either in residence or through extension courses in engineering schools.

Also there is need for the training of the imagination of engineers. Such education comes out of wide reading in the fields of literature and from the discussion of controversial issues; it comes from contact with other engineers and with those who practice other professions. It is time to begin to use the words, "engineering education," instead of the phrase, "engineering training." If the so-called training could be superimposed upon the education of the individual, wiser men would be developed in the profession. In modern technological civilization there is great need for the technicians called engineers, but, to master this mechanical civilization, technical men must be aware of the implications of modern life and they must feel at home in a changing world.

Note.—This paper by Donald M. Baker was published in April, 1946, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: September, 1946, by E. S. Boalich, Russell C. Brinker, I. Oesterblom, Samuel T. Carpenter, Lynn Perry, and L. E. Grinter; October, 1946, by Robert O. Thomas, Clement C. Williams, N. W. Dougherty, H. A. Wagner, M. E. McIver, and Scott B. Lilly; and December, 1946, by William A. Conwell, and A. Amirikian.

[36] Provost of the University, Univ. of California at Los Angeles, Los Angeles, Calif.

[36a] Received October 29, 1946.

A structural engineer should know more than how to build a bridge. He should be able to state whether a bridge is necessary, what will happen if it is built, and perhaps be able to suggest an alternate solution to what is traditionally thought of as a bridge problem.

Therefore, it is gratifying that the author is interested in the educational aspects of the engineering world and it is to be hoped he will continue to call attention to the fact that a study of engineers as well as a study of engineering is needed. After all, engineering schools are in the business of educating men, some of whom will be engineers for the remainder of their lives, and some of whom will find themselves with shifting and varying responsibilities which may be quite tangential to what is familiarly called the engineering profession. If, as it may happen, engineers are called much more frequently into management or into public life, the character and quality of their education becomes of real importance to the social structure.

Louis Balog,[37] Esq.[37a]—The major purpose of this inquiry as stated in the "Introduction" is: "* * * to evoke discussion of the subject of engineering education by the users of its product." The point of view of "the users of the product" does not necessarily coincide with public interest and with the professional interest of the individual. In fact the statistical data included in the paper indicate that these interests are quite divergent. The opinion expressed in the paper—that the assumption of administrative duties by an engineer constitutes professional advancement when he begins "depending on his subordinates more and more for technical results and opinions,"—misrepresents the value of engineering science. It would appear reasonable that work involving technical science should be rated above all other activities, and that superior technical knowledge should enable the engineer to assign subordinates to the nontechnical duties of the managerial position. Educational institutions and professional organizations should not create men prepared to prosper on the knowledge of subordinate engineers; rather, they should produce engineers who will have reasonable hope of improving their conditions by professional, and not by business, efforts.

A profession is a vocation that requires a learned education. A uniform educational standard, therefore, is a necessary requirement for creating a profession. This was well recognized in the 1920's by the American Medical Association in rating medical schools and in undertaking a relentless effort for the immediate closure of third-rate and, for the gradual elimination of all second-rate, institutions of medical instruction. As long as uniform educational requirements for the designation "engineer" are not established, the group classification of "engineering profession" remains meaningless. The divided authority of the university, the state registration board, and the engineering society in declaring individuals engineers, cannot result in a true professional status for the engineer, similar to that of medical men. The latter, upon complying with precisely defined scholastic requirements, are declared to

[37] Cons. Engr., New York, N. Y.

[37a] Received August 8, 1946.

be doctors of medicine on the strength of the authority of the university and become full-fledged members of their profession.

From statistical data, Mr. Baker concludes that the engineer at present cannot achieve professional status by education (see heading, "The Practice of Engineering: Rate of Advancement"):

> "* * * the average engineer—at least if he follows civil engineering—does not attain a position in which he is called on * * * to assume engineering responsibility for anything except work of the simplest character, until at least from eight to ten years after he has graduated; and many do not attain such a position within that time."

Under the heading, "Prewar Engineering Education," Mr. Baker states also that at the university the student: "* * * grappled with problems of a character that he would never meet, or at least would not have to solve, on his own responsibility for from at least twenty to twenty-five years after graduation." In striking contrast to the foregoing statements, Arthur J. Boase, M. ASCE, in reporting on the advanced stage of development of reinforced-concrete design in South American countries, observes that the leading designers of those countries are young men, the ages ranging from twenty-five to thirty-five years.[38] None of these highly educated, able designers (who have developed a reinforced-concrete design practice far superior to that of the level achieved in the United States) could satisfy Mr. Baker's criterion for engineering responsibility or be classified as able to design. This is an indication of something fundamentally wrong in the guiding principles that underlie his opinions concerning the purposes of civil engineering education and professional organization.

Effecting that professional education and technical knowledge, so that it will be to the best advantage of the individual and the community, is the foremost duty of any group of men engaged in the same occupation. The engineering society has been ineffective in these professional objectives primarily because it is not a group of persons engaged in the same occupation and interest. According to Mr. Baker's statistics (Table 4), 46% of its corporate membership is not engaged in engineering; and a closer scrutiny of the actual activity of those in the grade of member, by Arthur Wardel Consoer, M. ASCE, reveals that "* * * only a pitifully small group * * * can really consider themselves to be practicing engineering as a profession."[39] In demonstrating similar facts (Fig. 1), the author arrives at the conclusion that engineering education should emphasize only the fundamental principles of engineering and should include executive and managerial training. At the convention of the American Society for Engineering Education in June, 1946, the recommendation was made that civil engineering education should emphasize fundamentals and that design "can be better learned in practice." Such recommendations are not to the best interests of the civil engineer. If South American engineers, credited with outstanding accomplishments at the age of twenty-five years[38]

[38] "South American Building Is Challenging," by Arthur J. Boase, *Engineering News-Record*, Vol. 133, 1944, p. 121.

[39] "The Professional Status of Civil Engineers," by Arthur Wardel Consoer, *Civil Engineering*, November, 1939, p. 673.

had received the education advocated by Mr. Baker and the Americn Society for Engineering Education, they could never have achieved real engineering prominence; a few may have become managers of some mass-production organization "twenty to twenty-five years after graduation." Why should the young men of the United States be denied an education that would equip them for independent accomplishment as civil engineers?

In discussing the "Organization and Structure of the Engineering Profession" under the heading, "The Engineering Profession," Mr. Baker states: "The current practice of engineering, except by those who serve in advisory or consulting capacities, is a mass-production activity * * *." Critical examination of this correct observation reveals, however, that this condition involves all the disadvantages and none of the advantages of mass production, and that it is detrimental to the professional interest of the civil engineer. Engineering structures are not produced by mass-production methods, and nature seldom provides the same conditions twice. A structure that fits perfectly into its location always represents saving to balance the insignificant cost of a good design. Mass-production principles manifest themselves in standardization, and standardized designs are the most effective means of hindering progress. They minimize the value of talent in design and hinder the useful employment of engineers. The application of mass production to design has created the highly valued executive, but it has degraded the designer, and eliminated competitive design and, with it, the proper recognition of individual accomplishments, which is the prime requisite for professional existence.

The management of engineering organizations, at present, may be a purely parasitic activity in which mere trader's cunning, or certain dexterity in the use of technical terms, is sufficient qualification for a position above the engineer. The slight technical knowledge that is required for this form of management is demonstrated by the fact that most heads of engineering organizations advanced to their positions through salesmanship or assumed executive duties. The emphasis placed on management and organization, at the expense of technical knowledge, has lowered the designer to the status of a laborer. The user of the product, whose advice Mr. Baker seeks in an effort to improve engineering education, paid the highly educated designer (the true representative of the civil engineering profession) less than the wages of unskilled labor, and terminated his employment immediately upon completion of the work. These practices have forced designers to use the methods of union labor in their struggle for existence, a disgrace that removed from the civil engineer even the semblance of belonging to a learned profession. Mr. Baker accepts this as an unalterable situation and recommends that the student of engineering be advised as to what he is "getting into." According to Mr. Baker (under the heading, "Postwar Engineering Education"):

> "The student should also receive a realistic picture of the profession which he has chosen for a life career—not the glorified aspect usually presented by older alumni or distinguished engineers who address upper classmen at meetings."

It is of even greater importance to the student to know that the distinguished engineers are purposely not telling the truth. The incredibly cynical

admissions of this appalling fact, in private conversation, show the prevailing conditions in the civil engineering profession to be based on the betrayal of morality on which all human relations should rest.

The author's recommendations that the experience records of engineers be studied in order to discover needed educational improvements and the demand for engineering services can yield no useful results. Such records do not show the actual accomplishments or the capabilities of anyone; and the demand for engineering services is never revealed by such records.

Examination of engineering structures will indicate educational and professional conditions; it will indicate needed educational improvements and wasted opportunities for the employment of engineering services. The professional biographies of the engineers, enumerating their engagements will indicate nothing. Anywhere and everywhere, structures can be observed which disclose the need for engineering services—instances where the use of expert engineering talent would have resulted in savings, improved appearance, and increased engineering employment.

Mr. Baker's analysis of the advancement of engineers in the qualification grades of the engineering society, therefore, cannot be accepted as a suitable guide for the improvement of engineering education or professional organization. On the contrary, the misleading conclusions that result indicate that the rejection of his basic principles are a necessary requirement for the achievement of actual improvements.

The classification of engineers by interested private individuals, as "juniors," "associates," and "members," that postpones recognition of design ability until after a person has passed the prime of life, [40,41] and that declares any one older than thirty-five years able to design, irrespective of his actual occupation, is incompatible with the conception of a learned profession. The growth of technical sciences has resulted in the evolution of educational institutions that actually eliminate the system of apprenticeship established by this antiquated and wholly unjust classification of engineers by the engineering society. If this were not an artificially created situation, the university as an institution for educating civil engineers could be regarded as a complete failure. The inadequacy of the university is a direct result of the influence exerted by the controlling ideas in the existing organization of the civil engineering profesision.

The engineer will attain professional status when a properly constituted university has become recognized as the sole authority for qualifying an individual, upon completion of specified studies and examinations. Since the rights of individuals are guaranteed by the authority of the state, the state should establish the educational requirements leading to those rights and should be represented by its properly constituted registration board in the final examinations of engineers at the university. Such an arrangement eliminates duplication of effort. It eliminates the system of dependence on personal

[40] "Man's Most Creative Years," by Harvey C. Lehman, *The Scientific Monthly*, Vol. 59, 1944, pp. 384–393.

[41] "'Intellectual' Versus 'Physical' Peak Performance," by Harvey C. Lehman, *ibid.*, Vol. 61, 1945, pp. 127–137.

recommendations; it guarantees that engineers will be able to begin practicing their profession at a reasonable age and succeed according to their ability.

A professional man is necessarily a lifelong student whose knowledge should increase steadily. The admission to a profession, however, should be based on a precisely defined, formal education which preferably should include the entire educational period. The scholastic level of high schools in the United States is lower than that of preparatory schools of other countries of the world by at least two years of study.[42,43] A more efficient secondary education is of great interest to the engineer. He should be given the possibility of acquiring a proper cultural and basic scientific education and the formation of mental habits that result in self-dependence and maturity, before entering the university at the age of eighteen. Experience in education indicates that this can be achieved and a professional organization of engineers should influence the establishment of such an educational procedure.

It is rational to spend the first two years at the university with the basic and advanced theories now given as graduate study, the following two years with the application of theories to design, and the fifth year with specialized technical studies and courses in economics and statistics. This university program of five years should be based on at least 40 hours each week in lectures, laboratory, and design work. Upon completion of this work, and the examinations, the candidate should prepare a thesis and pass a written and oral examination in his three principal engineering subjects before a board comprising members of the university and federal registration authorities. This joint board should declare the successful candidate an engineer, and he should be acknowledged a full-fledged member of the profession at the age of twenty-four.

The academic prestige of the engineer's diploma should be the same as that of the doctor of medicine. Courses that do not require compulsory work and specific examinations by a duly formed combination of university and registration boards should be disqualified in appraising candidates for the degree of engineer. It is a truism that taking courses does not mean that the individual has any knowledge of the subjects involved.

The graduate engineer, upon presentation of a paper which is rated satisfactory by a special board of the university, and upon passing an oral examination, should be able to obtain the academic degree of doctor from the university. This degree, however, should not give rights in addition to those acquired with his diploma. The degrees of Bachelor of Science and Master of Science, in engineering, should not exist. All oral examinations should be public.

The sequence of studies described in this discussion is psychologically more sound for the development of the proper sense of values than the prevailing reversed procedure. Observation indicates that an elementary structural education, followed by the study of the theories of elasticity and stability, often results in an attitude that misjudges the rôle and value of mathematical pro-

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[42] "Rhodes Scholarships and American Scholars," by G. R. Parkin, *The Atlantic Monthly*, Vol. 124, pp. 365–375.

[43] "Secondary Education in the United States," by William A. Smith, The Macmillan Co., New York, N. Y., 1932, p. 108.

cedures in creating structures. The layout determines the cost, quality, and appearance of a structure. The performance is ascertained by analysis only; but analysis can never improve upon a structure if the layout is faulty. All the knowledge contained in the curricula is necessary in structural design. The subjects of design, therefore, must follow the mastering of advanced theories.

Mr. Baker's idea of transforming engineers into managers by the study of such a miscellany of subjects as "human nature," "institutions," and "business practice" should be rejected. Only superior technical knowledge should be the qualification for leading positions in engineering. The only nontechnical subjects required of the engineer should be economics and statistics. The analysis of scientifically classified collections of fact relating to national and world economy is a suitable method of interpreting conditions and values by technically trained minds.

As executive training or experience does not make one a designer, teaching experience does not make one a professor. Thorough technical knowledge of the subjects involved is the fundamental requirement in both cases. A thousand students would rather listen to the lectures of an outstanding teacher —than groups of fifty, to men of limited knowledge and faulty point of view. Personal contact between lecturer and students is not necessary. Self-dependence is essential for the development of the student. The professor should not be required to expend more time than that required for lectures and examinations.

During the first two years at the university, since the emphasis is on analysis, the engineer is trained by men who are primarily theorists. After the second year, the lectures should be given by practicing engineers. Lecturers should be required, concurrently, to engage in research or design, keep abreast of all progress in their respective lines, and modify their lectures every year. The compensation of lecturers and their assistants should be so high that everyone would strive to attain the knowledge that qualifies men for a position at the university.

The educational and qualifying procedures outlined in this discussion, and the establishment of free competition in design, are the only means for establishing the civil engineer in his proper professional status. A suggestion that, in order to succeed, the engineer should be a politician, an orator, an executive, a salesman, or anything but a person who applies technical science in his work, is an implicit admission that technical knowledge is of no value. This condition follows from the prevailing system of qualifying engineers through personal recommendations, by which means anyone can be made a great engineer or a lifelong slave. As long as it is possible to declare every technical function a matter of detail—as long as it is possible to accord credit for the work of others to anyone whose advancement is the personal interest of a user of the product—the existence of a civil engineering profession will remain a mere illusion.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

RELIEF WELLS FOR DAMS AND LEVEES

Discussion

BY JOHN S. MCNOWN

JOHN S. MCNOWN,[26] Assoc. M. ASCE.[26a]—For some time the writer has been interested in the application of mathematical methods to practical problems of fluid flow, and has contended that many existing mathematical treatments would find wider application if presented in a less obscure manner. It is important from the standpoint of clarity that symbols, wherever used, be clearly defined and used in an entirely consistent manner. Moreover, since the algebraic representation as applied to practical problems is merely a symbolic shorthand, the relationship represented by an equation should be expressed in words wherever the significance of the equation is not readily apparent.

The application of the methods of hydrodynamics to relief well design presented by the authors is an interesting analytical problem leading to worthwhile results. However, the development of the basic equations is exceedingly difficult for the reader to appraise and follow because (1) the explanation of principles underlying the development is far from complete, (2) the symbology is thoroughly confusing, and (3) several inaccurate or inconsistent statements have been included. Since the writer feels that the results obtained by the authors are significant, it seems worthwhile to clarify parts of the development and to reconcile some of the troublesome inconsistencies.

In any development of a highly mathematical nature, it is not the rigorous algebraic transformations which interest the practical-minded engineer, but rather the accuracy of the underlying assumptions and the acceptability of approximations so often essential to the obtainment of useful results. From this viewpoint, the omission of any discussion of the rather surprising analogy which permits the use of hydrodynamic theory for certain problems of viscous motion is regrettable. As presented, the material leaves the reader with no method of evaluating the usefulness of the final equations.. Much of the

NOTE.—This paper by T. A. Middlebrooks and William H. Jervis was published in June, 1946, *Proceedings.* Discussion on this paper has appeared in *Proceedings*, as follows: October, 1946, by Henry C. Barksdale, Willard J. Turnbull, and Glennon Gilboy; December, 1946, by W. A. Wall and C. A. Stone; and January, 1947, by John R. Charles, Horace A. Johnson, P. C. Rutledge, H. H. Roberts and Carter V. Johnson, Frank E. Fahlquist, and Harry R. Cedergren.

[26] Research Engr., Iowa Inst. of Hydr. Research, and Asst. Prof., Dept. of Mechanics and Hydraulics, State Univ. of Iowa, Iowa City, Iowa.

[26a] Received December 2, 1946.

mathematical development is unquestionably beyond the scope of the paper and, furthermore, has been presented elsewhere; nevertheless, the authors might fully as well omit the mathematics and present only the final equations, once they have omitted the initial steps leading to Eq. 7*a*.

In deriving this basic equation, it is necessary to define and to utilize briefly the concept of velocity potential in relating the flow of a frictionless fluid into mathematical sinks and the laminar flow of a real fluid into relief wells. A velocity potential φ is usually defined as a quantity whose negative derivative in any direction is the component of velocity in that direction—that is:

$$-\frac{\partial \phi}{\partial x} = V_x \qquad (26a)$$

Furthermore, several writers have shown[3,27,28] that the Navier-Stokes equations of viscous motion for flow with negligible acceleration reduce to the form:

$$-\frac{\partial h}{\partial x} = C\,V_x \qquad (26b)$$

in which h is the piezometric head and C is a dimensional constant of proportionality.

A comparison of Eqs. 26 demonstrates the analogy between these two types of motion and indicates that the piezometric head (multiplied by a proportionality factor) will serve as the velocity potential for flow through granular media. Because of this accidental parallel between two widely differing types of motion, many of the results and methods of hydrodynamics may be applied to problems of flow through porous media. Flow into a single well has its exact counterpart in the flow into a mathematical sink, and a number of wells can be studied by utilizing expressions already developed for flow into a comparable number of sinks similarly arranged. Although flow from a river into a line of wells is closely approximated by the corresponding expression for flow into a line of sinks, the condition of a straight line of uniform pressure (the edge of the river) may be exactly reproduced by placing a symmetrical line of sources or supply wells an equal distance the other side of the straight line of uniform pressure. The mathematical expression for the infinite lines of sources and sinks is initially quite complex, but it may be partly simplified to a usable form corresponding to Eq. 7*a*. Since piezometric heads are of more direct usefulness than pressure intensities, Eq. 7*a* has been rewritten to give the piezometric head h at the point with general coordinates (x,y) in the following form:

$$h = h_s + c \log_e \frac{\cosh j(y - s) - \cos j\,x}{\cosh j(y + s) - \cos j\,x} \qquad (27)$$

The numerator of the fraction in Eq. 27 is a mathematical simplification giving the sum of the velocity potentials for the sinks along $y = s$ and the denominator

[3] "The Flow of Homogeneous Fluids Through Porous Media," by Morris Muskat, McGraw-Hill Book Co., Inc., New York, N. Y., 1st Ed., 1937.

[27] "Fluid Mechanics for Hydraulic Engineers," by Hunter Rouse, McGraw-Hill Book Co., Inc., New York, N. Y., 1st Ed., 1938.

[28] "Hydraulik," by Phillipp Forchheimer, B. G. Teubner, Leipzig and Berlin, 1930.

for the sources at $y = -s$, s being therefore the distance between the edge of the river and the line of wells. The piezometric head along the edge of the river is designated by h_s. The x-axis coincides with the line of symmetry (the edge of the river), and the y-axis passes through one of the wells. The substitution $j = 2\pi/a$, in which a is the well spacing, has again been used to simplify typography. The coefficient c, which reflects both the quantity of flow into each well and the characteristics of the medium and the fluid, may be defined as

$$c = \frac{\mu Q}{4 \pi k \gamma} \qquad (28)$$

in which the dynamic viscosity μ characterizes the fluid resistance; and Q is the rate of discharge into each well (the vertical depth of the stratum being assumed as unity). The permeability of the medium k is defined by Mr. Muskat[29] as "the volume of a fluid of unit viscosity passing through a unit cross section of the medium in a unit time under the action of a unit pressure gradient" and is entirely determined by the geometry of the medium. The value of k reflects the ability of the medium to transmit fluid much as a coefficient of heat transfer reflects ability to transmit heat. The unit weight of the liquid, γ, has been introduced to balance the units of Eq. 27. These equations differ somewhat from Eqs. 7 because the writer found it impossible to follow the conflicting definitions and units and still to arrive at a correct equation.

Together with these fundamental relationships, a restatement of the problem in view is essential. Expressions are sought relating (a) the well discharge to the drop in head between the river and the well, and (b) the head midway between adjacent wells to that at the wells. It is necessary, therefore, to evaluate from Eq. 27 the piezometric head at the edge of the well and midway between wells in terms of known or measurable quantities. If the radius of the well r_w is assumed to be small relative to the well spacing (as it is in any practical case), the lines of constant pressure in the region of the well are essentially circular, and the piezometric head at the well may be evaluated at any point around the periphery. The pressure cannot be evaluated at the center of the well, because the medium is discontinuous in this region in direct contrast to the assumption underlying the derivation. Therefore, if the coordinates (r_w,s) are substituted in Eq. 27, an expression is obtained for h_w, the head in the well:

$$h_w = h_s + c \log_e \frac{1 - \cos j\, r_w}{\cosh 2\, j\, s - \cos j\, r_w} \qquad (29a)$$

In the numerator of the fraction $j\, r_w$ is small, so that $\cos j\, r_w$ may, with good approximation, be replaced by the first two terms of the equivalent series expansion, $1 - \frac{1}{2}(j\, r_w)^2$. Also, since $2\, j\, s$ is considerably greater than unity, $\cosh 2\, j\, s$ may be replaced by $\frac{1}{2}\, e^{2js}$ and $\cos j\, r_w$ may be neglected in the denominator. With these readily permissible simplifications,

$$h_s - h_w = 2\, c\, (j\, s - \log_e j\, r_w) \qquad (29b)$$

If the coordinates $(a/2,s)$, corresponding to a point midway between the wells at $(0,s)$ and (a,s), are substituted in Eq. 27, the piezometric head at this

[29] "The Flow of Homogeneous Fluids Through Porous Media," by Morris Muskat, McGraw-Hill Book Co., Inc., New York, N. Y., 1st Ed., 1937, p. 71.

point can also be determined. In the absence of consistent notation, the symbol h_m will be assigned to this head, which can be referred to either the head at the river or the head in the wells by using Eqs. 27 and 29—that is:

$$h_s - h_m = 2\,c\,(j\,s - \log_e 2) \quad \text{(30a)}$$

or

$$h_m - h_w = 2\,c \log_e \frac{2}{j\,r_w} \quad \text{(30b)}$$

Finally, from Eqs. 29*b* and 30*b* the ratio presented as the ordinate scale in Fig. 4(*a*) can be quite simply expressed as follows:

$$\frac{h_m - h_w}{h_s - h_w} = \frac{\log_e (j\,r_w/2)}{\log_e j\,r_w - j\,s} \quad \text{(31a)}$$

For purposes of computation, Eq. 31*a* can be simplified even further by substituting the value of j and changing to decimal logarithms:

$$\frac{h_m - h_w}{h_s - h_w} = \frac{\log_{10}\left(\dfrac{a}{\pi\,r_w}\right)}{2.73\,\dfrac{s}{a} + \log_{10}\left(\dfrac{a}{2\,\pi\,r_w}\right)} \quad \text{(31b)}$$

Also, the combination of Eqs. 28 and 29 yields a relationship between the drop in head from the river to the well and the discharge per well:

$$Q = \frac{2\,\pi\,k\,(h_s - h_w)\,\gamma}{\mu\,(j\,s - \log_e j\,r_w)} \quad \text{(32)}$$

Eqs. 31 and 32 correspond to Eqs. 11 and 12, but are presented in consistent units and in a more readily usable form.

The foregoing development has been included in some detail, in part because the parallel treatment by the authors lacked a systematic and readily understandable order, but primarily because throughout their development the authors have consistently departed from good practice in use of symbols. Although the results of the analysis as embodied in Fig. 4 are correct, it is typical of the confusion confronting the reader that the same quantity is designated as h in one part of Fig. 4 and as $h_s - h_w$ in the other. In fact, four different symbols were used at various points in the authors' development to represent this difference in head—namely, p, Δp, h, and $h_s - h_w$. The symbol p, furthermore, was assigned four distinct meanings, denoting the piezometric head at the point (x,y) in Eq. 7*a*, the difference in head $h_s - h_w$ a few lines thereafter, the unit pressure (actually piezometric head) at the midpoint between the wells in Eq. 8, and the difference $h_m - h_w$ in Fig. 8(*b*). As a result, the reader is forced to use intuitive as well as logical reasoning in following the authors' development.

Although the essential results given by the authors have proved to be correct, several omissions and points of inconsistency further detract from the presentation. It is difficult to obtain a satisfactory interpretation of Table 1 in the absence of specific values for s, h_s, and h_w.

The definition of the quantity "extra length," given as part of the discussion of Fig. 7, is not compatible with its representation in Figs. 7 and 8 and Eq. 13.

Neither theory nor electric analogy are suitable for evaluating the effect of a well screen. As presented, this length term is the difference between the length of the direct path from the river to the line of wells and an effective length obtained by dividing the total drop in head $h_s - h_w$ by the piezometric gradient evaluated some distance away from the line of wells. Introduction of this residual quantity leads to the useful plot (Fig. 8(*a*)) which is independent of the variable s (or s/a). However, since Fig. 8(*a*) yields only the extra length and Eq. 13 is necessary for the evaluation of Q, the word "flow" in the caption of Fig. 8 seems inappropriate. If this extra length is designated as b, it may be defined in relative form as

$$\frac{b}{a} = \frac{1}{2\,\pi} \log_e \frac{a}{2\,\pi\,r_w} \qquad (33)$$

which corresponds to the line for 100% penetration in Fig. 8(*a*). The equation for the related curve in Fig. 8(*b*) can be written in a like manner if it is noted that the value p in the ordinate scale is intended to signify the difference in head $h_m - h_w$ and that the porosity factor includes not only the effect of the porous material but the viscosity and unit weight of the water as well. If the writer's terminology and Mr. Muskat's definition of porosity are used, Eqs. 28 and 30*b* can be combined to represent the 100% line in Fig. 8(*b*)—that is:

$$\frac{(h_m - h_w)\,k\,d}{Q\,\gamma\,\mu} = \frac{1}{2\,\pi} \log_e \frac{a}{\pi\,r_w} \qquad (34)$$

It has been assumed that the effective radius of the well r_e is interchangeable with r_w in the absence of a definition of r_e or a relationship between the two.

The foregoing discussion and the restatement of the analytical part of the authors' paper were undertaken in an attempt to improve and correct the form of presentation. It is hoped that, in so doing, this section of the general discussion has been brought up to the high level of the remainder of the paper.

Corrections for *Transactions:* On page 786, Eq. 7*a* change "*d*" to "*s*" twice; and, on page 787, in line 3, transpose ",*k*" to "*k*," and change "< to ≪," in Eq. 7*b* change "*k*" to "*K*," in line 5 change "*x y*" to "(*x*,*y*)," in Eq. 7*c* change "*k*" to "*K*," in line 13 change "$x = \pm a \pm n\,a$" to "$x = \frac{a}{2} \pm n\,a$," in line 14, change "$x = a$" to "$x = \frac{a}{2}$," in line 16 change "Eq. 3" to "Eq. 7*a*," and in the right-hand side of Eq. 11 change "$1 + \dfrac{\log_e \dfrac{2}{1 + \cosh 2\,j\,s}}{2 \log_e \dfrac{e^{js}}{j\,r_w}}$" to "$1 + \dfrac{\log_e \dfrac{2}{1 + \cosh 2\,j\,s}}{2 \log_e \dfrac{e^{js}}{j\,r_w}}$." On page 792, change the first complete sentence to read: "The inflow region was represented by a solid sheet of copper in one end of the trough and the well by a copper wire, sized to scale, and a series of tests made by varying the wire size and the position of the wire to represent values of s/a from 3 to 10 and values of a/r_w from about 50 to 500." In the title of Table 1 add: "(25% penetration)"; and the head of the second column should read, "Nominal size of well (in.)." In Fig. 2 change d_e to H.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

STRENGTH OF BEAMS AS DETERMINED BY LATERAL BUCKLING

Discussion

BY THEODORE R. HIGGINS

THEODORE R. HIGGINS,[28] M. ASCE.[28a]—The author is to be complimented for his method of attack, which consisted, first, of solving the problem as rationally as practicable in view of the mathematical complexities and, second, in transforming the results into simple functions of published beam properties. There is obvious logic in a formula which considers, in addition to the length and width of the beam as embraced in formulas used heretofore, also the thickness of the flanges (which makes for lateral stability) and the beam depth (which militates against it). It is equally obvious that the four quantities, l, d, b, and t, appear in the author's formula in the numerator and denominator, respectively, as they properly should to express their influence upon lateral and torsional stability. The form of the author's formula is, accordingly, logical.

It is interesting to note, from the quantitative standpoint, that this formula operates to reduce the rated capacity of deep or narrow beams and to increase the rated capacity of shallow beams, as compared to their capacity when rated by previous specification formulas involving only l and b.

Although the case for Eq. 7, pertaining to long laterally unsupported beams, has convincingly been developed by the author, both analytically and statistically, the supporting argument for Eqs. 6*a*, 6*b*, and 6*c* is lacking. These equations are presented merely as affording "transition curves" with yield point stress as their upper limits. The analogy between the Euler curve for long columns, and Eq. 7, as applied to long laterally unsupported beams, is readily apparent. It does not follow directly, however, that a curve for short beams needs to simulate that for short columns as given by the secant formula.

As will be perceived by dividing 20,000,000 by 600, the critical bending stress in a beam having a $\frac{l\,d}{b\,t}$-value of 600 is not less than 33,333 lb per sq in.,

NOTE.—This paper by Karl de Vries was published in September, 1946, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: December, 1946, by George Winter, and David B. Hall.

[28] Director of Eng., Am. Inst. of Steel Constr., New York, N. Y.

[28a] Received January 2, 1947.

even under the most severe of the six different types of loading considered. Thus, the usefulness of a further reduction, amounting to $0.0125 \times (600)^2 =$ 4,500 lb per sq in., is not apparent in applying the results of the analysis to design problems.

From a practical standpoint, it is highly desirable to limit the use of variable working stresses—which impose an added burden on the designer—as much as possible, consistent with sound practices.

For these reasons the American Institute of Steel Construction (A.I.S.C.) Committee on Specifications has seen fit, in revising the Standard Specifications,[29] to recommend the use of Eq. 9 for all values of $\frac{l\,d}{b\,t}$ greater than 600, the point where the equation affords a working stress of 20,000 lb per sq in. For values of $\frac{l\,d}{b\,t}$ less than 600, the basic 20,000 lb per sq in. bending stress governs.

On this basis, for most rolled wide flange beams there is a rather substantial length up to which no reduction in capacity need be made because of the lack of lateral support. For instance, for a beam 36 in. deep by 16½ in. wide at 300 lb per lin ft, this length is 38 ft. Accordingly, in the Fifth (1946) Edition of the A.I.S.C. manual on "Steel Construction" in the tables of safe loads on beams, the constant value $\frac{d}{b\,t}$, and the length (L_u) up to which the absence of lateral support does not reduce the allowable load, are given for each beam size. For any given beam, the only variable in Eq. 9 is the factor l and, when $l > L_u$,

$$f = \frac{20{,}000\, L_u}{l} \qquad (65)$$

Although Eq. 9 determines the allowable unit stress once the beam has been selected, it requires (as did the previous l/b formula) the selection of a trial beam. To avoid this step, since three beam dimensions affect the allowable stress instead of one, the A.I.S.C. manual now provides four new charts from which proper beams can be selected without any "cut and try." The charts are so arranged that, entering on the left with the desired bending moment and at the bottom with the unsupported span length, and proceeding across and up, respectively, to the intersection point thus found, the curves for all beams which satisfy the new formula lie above and to the right of this point.

In applying Eq. 9 to beams where the $\frac{l\,d}{b\,t}$-value requires a reduction from the 20,000 lb per sq in. basic working stress, investigation of vertical deflection for all ordinary conditions is unnecessary. As the laterally unsupported span length increases, the reduction in allowable unit stress operates to an extent such that the vertical deflection will always be within the usual limits prescribed for a fully loaded, fully supported beam of the same span.

[29] "Specification for the Design, Fabrication and Erection of Structural Steel for Buildings," A.I.S.C., New York, N. Y., 1946.

The author has wisely called attention to the fact that the working formula (Eq. 9) is safe for the most severe of the six types of loading considered, and that in investigating special problems the designer would do well to consider the expressions derived for the other five conditions. For the ordinary run of work a single rule, safe for all conditions of loading, will suffice and will not prove wasteful of material. In studies of this kind, however, there is a natural tendency to lose sight of the information pertaining to special cases; and to ascribe to the single recommendation, intended to cover the general case, a sanctity not intended by the originator. These same considerations would suggest that the user of Eq. 9, in attacking problems involving unusual profiles, would do well to study carefully the discussion of some of these problems contained in the paper.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

DRAWDOWN TEST TO DETERMINE EFFECTIVE RADIUS OF ARTESIAN WELL

Discussion

BY R. M. LEGGETTE, AND M. R. LEWIS

R. M. LEGGETTE,[13] AFFILIATE, ASCE[13a]—Although it covers a highly technical subject, this paper clearly demonstrates the practical importance of a number of factors of well design. It seems desirable to emphasize these practical considerations because they are often given too little attention. Frequently water works men and well-drilling contractors greatly belittle or fail to recognize the magnitude of what Mr. Jacob calls "well loss."

It is obvious, of course, that the water level in a pumping well must be lower than the water level immediately outside the well. In many wells, much of this difference in head is screen friction loss which results from the use of a poorly designed screen. This difference in head is sometimes presumed to be only a few inches, or a fraction of a foot; however, actual observations have shown that in some wells the well loss is a considerable part of the total drawdown. Thus, from the point of view of economy of operation, well loss may be an important factor.

The paper also indicates the desirability of increasing the effective radius of a "sand and gravel" well by development to remove the fine material surrounding the screen, or by artificial gravel packing. It should be noted that the advantages of development or gravel packing may be largely overcome if an inefficient well screen is used.

The process of development by surging, swabbing, and brushing is being used more and more in uncased wells (rock wells), the walls of which apparently become "mudded up" during the drilling process. This clogging of the uncased wall of the well has the same effect as an inefficient well screen in a "sand and gravel" well. The well loss in many wells of this type has been greatly reduced by development.

NOTE.—This paper by C. E. Jacob was published in May, 1946, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: November, 1946, by N. S. Boulton; and December, 1946, by Carl Rohwer.

[13] Cons. Ground-Water Geologist, New York, N. Y.

[13a] Received November 21, 1946.

From the point of view of economy of pumping well water, this paper indicates the following: A well should be of large diameter; it should be extensively developed; and, if a well screen is used, it should be designed so as to produce a minimum of screen friction loss.

M. R. Lewis,[14] M. ASCE.[14a]—A valuable method has been presented, in this paper, for analyzing the capacity of artesian wells in spite of the necessary assumptions that there is uniformity in the aquifer and that the total supply to the well is drawn from the aquifer by the release of elastic forces. Such theoretical or mathematical treatments of the flow of fluids assist greatly in the understanding of practical problems even though the latter seldom are based on ideal conditions.

The multiple-step test proposed by the author should give very useful information on the points mentioned in the "Summary" wherever the assumptions are approximately fulfilled. It is hoped that the author will explore the possibility of a similar analysis under other conditions. Two important types of wells that might be studied are those of a simple water-table situation and those in which the bed overlying an aquifer is relatively impermeable but permits recharge from the soil surface surrounding the well.

It appears to the writer that two other factors besides the compaction of the aquifer are important elements in making the "apparent compressibility, β'," greater than the compressibility of water, β. These are (*a*) the increase in volume of the solid material of the aquifer because of the reduced hydrostatic pressure and (*b*) the reduction in the pore space because of the deformation of the solid particles by reason of the increased pressure on the mineral skeleton. In his earlier paper,[8] the author mentioned these factors but, apparently, considered them to be of negligible importance. Whether they are, or are not, important makes no difference in the author's analysis.

[14] Irrig. Engr., Bureau of Reclamation, Boise, Idaho.

[14a] Received November 29, 1946.

[8] "On the Flow of Water in an Elastic Artesian Aquifer," by C. E. Jacob, *Transactions*, Am. Geophysical Union, 1940, Pt. II, pp. 574–586.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

MATRIX ANALYSIS OF CONTINUOUS BEAMS

Discussion

BY I. OESTERBLOM, AND HARRIS SOLMAN

I. OESTERBLOM,[5] M. ASCE.[5a]—The use of matrices in structural engineering as presented in this paper constitutes a well-arranged review of the basic ideas and also a few sensible applications. Some matrix applications are already well known to engineers who have analyzed their continuity problems by the aid of energy, deflection, or slope methods, because all these, ultimately, lead to a system of equations for which a solution is possible only by the aid of determinants. (Among the many writers in this field, the following are noteworthy for comprehensive coverage and accessibility in American engineering libraries: Max Foerster,[6] David Molitor,[7] and J. I. Parcel and G. A. Maney.[8])

Too many engineers, however, have kept aloof from these methods, perhaps because they have been frightened by the unknown simplicity of matrix operations. Since the publication of this paper, however, they can no longer offer this as an excuse. On the other hand, others—the physics-minded type—have been led in the opposite direction. Albert Einstein's "generalized relativity" opened the entire field of physics to tensor operations, and this has led even to problems of elasticity where today, especially the French are offering most interesting simplifications[9] to some of the old and most discouraging problems; and tensors are nothing but the supreme of matrices, highly rationalized.

Perhaps there was also something beyond the matrices that frightened the early designers of indeterminate structures. It might not have been their presence but their extent. The nodal points in most commercial structures were many, as were, therefore, the elements in the matrix. It required a staff of good mathematicians under very keen leadership to set up the equations,

NOTE.—This paper by Stanley U. Benscoter was published in October, 1946, *Proceedings*.

[5] Engr., Carbide & Carbon Chemicals Corp., South Charleston, W. Va.

[5a] Received December 19, 1946.

[6] "Taschenbuch für Bauingenieure," edited by Max Foerster, Vol. I, Julius Springer, Berlin, 1928, pp. 309–316.

[7] "Kinetic Theory of Engineering Structures," by David Molitor, McGraw-Hill Book Co., Inc., New York, N. Y., 1911.

[8] "Statically Indeterminate Structures," by J. I. Parcel and G. A. Maney, John Wiley & Sons, Inc., New York, N. Y., 1936.

[9] "Les Tenseurs en Mécanique et en Élasticité," by Leon Brillouin, Masson et Cie, Paris, 1938; American edition by Dover Publications, New York, N. Y., 1946.

and then to solve them, for five stories and three spans only—not to mention the many larger frames. A maximum of fifteen redundants was as much as one man could handle.

The utmost in this respect was discovered in the Zeppelin frames—with one hundred or more nodal points.[10] When these airborne visitors, on destruction bent, arrived over the British Isles during World War I fear came to every man's heart: "What can we do to master these wicked monsters?" R. V. Southwell in England and J. C. Hunsaker in the United States worked on the problem, intensely and fearfully, to build better airships. Mountain high matrices as wide as an ocean—or so it must have seemed to those who did the work—were required.

Mr. Southwell's magnificent work now reposes in the archives of the Royal Society in London—a bewildering array of mass matrices of group elements; and a child was born out of this labor, too—the magnificent but ill-fated dirigible R101 which went to its destruction in the mountains of eastern France on its maiden trip to India. This was the only airship that was ever truly and rationally designed—so the world found when peace came. As a measure necessary to prove that the war was truly over, the foremost "matricians" of the world were ordered to surrender all their designs and calculations for the Zeppelins. It was a sorry collection they presented. The master mathematicians had been defeated by too many nodal points; they had nothing to show except reams of empiricism heavily loaded with guesswork.

This leads one to ask: "What good today are the matrices in dealing with frameworks?" If the latter are complicated, matrices are inadequate, and if they are simple, the modern relaxation methods are as good for the simple tasks as they are for the most complicated—and much simpler in application.

The engineer must think further than that, however, even though Mr. Benscoter's applications have been somewhat limited, for there are other problems—as in the field of dynamics and elasticity—many of them not yet born. When these rise out of the twilight of the unknown, designers will be thankful to Mr. Benscoter for providing such a fine light to lead them through the darkness.

Quite incidentally, the paper reveals that the stiffness of continuity may be expressed as a power series. Is this for a single sequence only, or does it apply also for a frame in two or three dimensions? For such a frame the discovery would have an epochal significance, because it would lighten the burden considerably when one has to deal with changes of sections or loads in a frame. As things are now, much guessing must be done because the actual work of rational redesign would be prohibitive for most such problems, because of the time required.

HARRIS SOLMAN,[11] M. ASCE.[11a]—By using the theory of matrices in the analysis of continuous beams, the author has made a valuable contribution to the field of structural analysis. The presentation of relationships in the form

[10] *Popular Aviation*, January, 1934, p. 29.

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[11a] Received December 20, 1946.

of a matrix or a system of matrices not only has the advantage of a convenient, systematic, and easily visualized arrangement but also possesses a mathematical elegance, which—it is hoped—engineers will not be too "practical" to appreciate.

Aside from the theoretical interest of the author's method, however, there is also a practical feature involved which has not been sufficiently emphasized in the paper. The method presented can be utilized conveniently in setting up general values of end moments for a given system of continuous beams in terms of loads, so that they can be useful in computing influence coefficients.

A number of years ago the writer became interested in setting up general formulas to express the end moments of continuous beams on supports fixed against translation in terms of the fixed-end moments. Since a load placed at any one point on the structure produces only two fixed-end moments (one at each end of the loaded span), only two terms of each such formula would need to be evaluated for any one position of the load. Furthermore, the coefficient for each term would be a function of the beam constants only and would be an invariant for all positions of the load. Such formulas were deduced and successfully used by the writer.

These formulas, transcribed in the same general notations as those used by the author, are given subsequently. The following minor changes in notation are to be observed: The first end support (noneffective) was designated as zero, the symbol n being reserved to indicate the last effective support (the degree

Description	1		2		n	
Fixed-end moments	M'_{10}	M'_{12}	M'_{21}	M'_{23}	$M'_{n(n-1)}$	$M'_{n(n+1)}$
Distribution factors	d_{10}	d_{12}	d_{21}	d_{23}	$d_{n(n-1)}$	$d_{n(n+1)}$
Carry-over factors	0	r_{12}	r_{21}	r_{23}	$r_{n(n-1)}$	0
q-factors ($d \times r$)	0	q_{12}	q_{21}	q_{23}	$q_{n(n-1)}$	0

FIG. 8.—BEAM CONSTANTS FOR AN n-DEGREE SYSTEM

of the system). Additional symbols p and Δ were introduced for convenience and are defined by the following relationships:

$$p_{uv} \text{ (for } 0 < u \leqq n \text{ and } 0 > v \geqq n) = q_{uv}\, q_{vu} \quad \ldots\ldots\ldots (130a)$$

and, for an n-degree system:

$$\Delta_n = \text{determinant} \begin{vmatrix} 1 & q_{12} & 0 & \cdots & 0 \\ q_{21} & 1 & q_{23} & \cdots & 0 \\ 0 & q_{32} & 1 & \cdots & 0 \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ 0 & 0 & 0 & \cdots & 1 \end{vmatrix} \quad \ldots\ldots\ldots (130b)$$

The beam convention for the algebraic sign is used throughout, in order that the formulas may be readily applied to the beams with their proper signs without the necessity of considering the rotation effects on the joints.

In general, for an n-degree system (see Fig. 8):

$$\left.\begin{aligned}
M_{10} &= M'_{10} + b^{(n)}_{11}\,(M'_{10} - M'_{12}) + b^{(n)}_{12}\,(M'_{21} - M'_{23}) + \cdots \\
&\qquad + b^{(n)}_{1n}\,[M'_{n(n-1)} - M'_{n(n+1)}] \\
M_{12} &= M'_{12} + a^{(n)}_{11}\,(M'_{10} - M'_{12}) + a^{(n)}_{12}\,(M'_{21} - M'_{23}) + \cdots \\
&\qquad + a^{(n)}_{1n}\,[M'_{n(n-1)} - M'_{n(n+1)}] \\
M_{21} &= M'_{21} + b^{(n)}_{21}\,(M'_{10} - M'_{12}) + b^{(n)}_{22}\,(M'_{21} - M'_{23}) + \cdots \\
&\qquad + b^{(n)}_{2n}\,[M'_{n(n-1)} - M'_{n(n+1)}] \\
M_{23} &= M'_{23} + a^{(n)}_{21}\,(M'_{10} - M'_{12}) + a^{(n)}_{22}\,(M'_{21} - M'_{23}) + \cdots \\
&\qquad + a^{(n)}_{2n}\,[M'_{n(n-1)} - M'_{n(n+1)}] \\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\
M_{n(n-1)} &= M'_{n(n-1)} + b^{(n)}_{n1}\,(M'_{10} - M'_{12}) + b^{(n)}_{n2}\,(M'_{21} - M'_{23}) + \cdots \\
&\qquad + b^{(n)}_{nn}\,[M'_{n(n-1)} - M'_{n(n+1)}] \\
M_{n(n+1)} &= M'_{n(n+1)} + a^{(n)}_{n1}\,(M'_{10} - M'_{12}) + a^{(n)}_{n2}\,(M'_{21} - M'_{23}) + \cdots \\
&\qquad + a^{(n)}_{nn}\,[M'_{n(n-1)} - M'_{n(n+1)}]
\end{aligned}\right\} \quad \ldots(131)$$

For $n = 1$, coefficient $b^{(1)}_{11} = -d_{10}$ and coefficient $a^{(1)}_{11} = d_{12}$ so that Eqs. 131 reduce to:

$$\left.\begin{aligned}
M_{10} &= M'_{10} - d_{10}\,(M'_{10} - M'_{12}) \\
M_{12} &= M'_{12} + d_{12}\,(M'_{10} - M'_{12})
\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots(132)$$

For $n = 2$, Eqs. 131 reduce to

$$\left.\begin{aligned}
M_{10} &= M'_{10} - \frac{d_{10}}{\Delta_2}\,(M'_{10} - M'_{12}) + \frac{q_{21}\,d_{10}}{\Delta_2}\,(M'_{21} - M'_{23}) \\
M_{12} &= M'_{12} + \frac{d_{12} - p_{12}}{\Delta_2}\,(M'_{10} - M'_{12}) + \frac{q_{21}(1 - d_{12})}{\Delta_2}\,(M'_{21} - M'_{23}) \\
M_{21} &= M'_{21} - \frac{q_{12}(1 - d_{21})}{\Delta_2}\,(M'_{10} - M_{12}) - \frac{d_{21} - p_{12}}{\Delta_2}\,(M'_{21} - M'_{23}) \\
M_{23} &= M'_{23} - \frac{q_{12}\,d_{23}}{\Delta_2}\,(M'_{10} - M'_{12}) + \frac{d_{23}}{\Delta_2}\,(M'_{21} - M'_{23})
\end{aligned}\right\} \quad \ldots(133)$$

For the general n-degree system:

$$\left.\begin{aligned}
&b^{(n)}_{uv} \text{ (for } 0 < u < n \text{ and for } 0 < v < n) = b^{(n-1)}_{uv} + b^{(n-1)}_{u(n-1)} \\
&\qquad\qquad \times a^{(n-1)}_{(n-1)v}\, r_{(n-1)n}\, q_{n(n-1)} \frac{\Delta_{(n-1)}}{\Delta_n} \\
&b^{(n)}_{un} \text{ (for } 0 < u < n) = -\, b^{(n-1)}_{u(n-1)}\, q_{n(n-1)} \frac{\Delta_{(n-1)}}{\Delta_n} \\
&b^{(n)}_{nv} \text{ (for } 0 < v < n) = -\, a^{(n-1)}_{(n-1)v}\, r_{(n-1)n} [1 - d_{n(n-1)}] \frac{\Delta_{(n-1)}}{\Delta_n} \\
&b^{(n)}_{nn} = \frac{p_{(n-1)n}\, \Delta_{(n-2)} - d_{n(n-1)}\, \Delta_{(n-1)}}{\Delta_n} = \frac{[1 - d_{n(n-1)}]\, \Delta_{(n-1)}}{\Delta_n} - 1 \\
&a^{(n)}_{uv} \text{ (for } 0 < u < (n-1) \text{ and } 0 < v < n) = a^{(n-1)}_{uv} \\
&\qquad\qquad + a^{(n-1)}_{u(n-1)}\, a^{(n-1)}_{(n-1)v}\, r_{(n-1)n}\, q_{n(n-1)} \frac{\Delta_{(n-1)}}{\Delta_n} \\
&a^{(n)}_{un} \text{ (for } 0 < u < (n-1)) = -\, a^{(n-1)}_{u(n-1)}\, q_{n(n-1)} \frac{\Delta_{(n-1)}}{\Delta_n} \\
&a^{(n)}_{(n-1)v} \text{ (for } 0 < v < n) = a^{(n-1)}_{(n-1)v} + [a^{(n-1)}_{(n-1)(n-1)} - 1] \\
&\qquad\qquad \times a^{(n-1)}_{(n-1)v}\, r_{(n-1)n}\, q_{n(n-1)} \frac{\Delta_{(n-1)}}{\Delta_n} \\
&a^{(n)}_{nv} \text{ (for } 0 < v < n) = -\, a^{(n-1)}_{(n-1)v}\, r_{(n-1)n}\, d_{n(n+1)} \frac{\Delta_{(n-1)}}{\Delta_n} \\
&a^{(n)}_{(n-1)n} = [1 - a^{(n-1)}_{(n-1)(n-1)}]\, q_{n(n-1)} \frac{\Delta_{(n-1)}}{\Delta_n} \\
&a^{(n)}_{nn} = d_{n(n+1)} \frac{\Delta_{(n-1)}}{\Delta_n}
\end{aligned}\right\} \quad ..(134)$$

Eqs. 134 operate by providing a procedure for building up the coefficients of a higher degree system from those of the lower degree system. Eqs. 132 and 133 for $n = 1$ and $n = 2$ may serve as the initial step in the progressive building up of the coefficients. These formulas are equivalent to the following matrix equations suggested in the author's presentation:

$$\begin{pmatrix} a^{(n)}_{11} & a^{(n)}_{12} \cdots & a^{(n)}_{1n} \\ a^{(n)}_{21} & a^{(n)}_{22} \cdots & a^{(n)}_{2n} \\ \cdots & \cdots & \cdots \\ a^{(n)}_{n1} & a^{(n)}_{n2} \cdots & a^{(n)}_{nn} \end{pmatrix} = \begin{pmatrix} d_{12} & q_{21} & 0 & \cdots 0 \\ 0 & d_{23} & q_{32} & \cdots 0 \\ 0 & 0 & d_{34} & \cdots 0 \\ \cdots & \cdots & \cdots & \cdots \\ 0 & 0 & 0 & \cdots d_{n(n+1)} \end{pmatrix} \begin{pmatrix} 1 & q_{21} & 0 & \cdots 0 \\ q_{12} & 1 & q_{32} & \cdots 0 \\ 0 & q_{23} & 1 & \cdots 0 \\ \cdots & \cdots & \cdots & \cdots \\ 0 & 0 & 0 & \cdots 1 \end{pmatrix}^{-1} \quad ..(135a)$$

and

$$\begin{bmatrix} b^{(n)}_{11} & b^{(n)}_{12} \cdots b^{(n)}_{1n} \\ b^{(n)}_{21} & b^{(n)}_{22} \cdots b^{(n)}_{2n} \\ \cdots & \cdots \\ b^{(n)}_{n1} & b^{(n)}_{n2} \cdots b^{(n)}_{nn} \end{bmatrix} = - \begin{pmatrix} d_{10} & 0 & 0 & \cdots 0 \\ q_{12} & d_{21} & 0 & \cdots 0 \\ 0 & q_{23} & d_{32} & \cdots 0 \\ \cdots & \cdots & \cdots & \cdots \\ 0 & 0 & 0 & \cdots d_{n(n-1)} \end{pmatrix} \begin{pmatrix} 1 & q_{21} & 0 & \cdots 0 \\ q_{12} & 1 & q_{32} & \cdots 0 \\ 0 & q_{23} & 1 & \cdots 0 \\ \cdots & \cdots & \cdots & \cdots \\ 0 & 0 & 0 & \cdots 1 \end{pmatrix}^{-1} \quad \text{..(135}b\text{)}$$

or, in abbreviated symbol form,

$$[a] = [K_a][D]^{-1}\{[1] - [Q']\}^{-1} \text{.............(136}a\text{)}$$

and

$$[b] = -[K_b][D]^{-1}\{[1] - [Q']\}^{-1} \text{.............(136}b\text{)}$$

The author proceeds to break up the matrix $\{[1] - [Q']\}^{-1}$ into a summation of successive approximations—$[1] + [Q'] + [Q']^2 + [Q']^3 + [Q']^4 + \cdots$.

Although this development is interesting from the standpoint of a theoretical discussion, the writer questions the advantage of such a further transformation, since it changes an absolute value for each coefficient to an approximation, for which the degree of accuracy in each case must be predetermined. Furthermore, the breaking up of the reciprocal matrix into a summation is based on the application of Eq. 97; but that relationship has a condition of convergence imposed upon it and is known not to apply to some mathematical units, as for instance to negative numbers and to complex numbers. Its application in this instance, therefore, would have to be substantiated by a rigorous proof.

Fortunately, it is not difficult to set up a matrix equal to the original reciprocal matrix without resolving it into a summation:

$$\{[1] - [Q']\}^{-1} = \begin{pmatrix} 1 & q_{21} & 0 & 0 & \cdots 0 & 0 \\ q_{12} & 1 & q_{32} & 0 & \cdots 0 & 0 \\ 0 & q_{23} & 1 & q_{43} & \cdots 0 & 0 \\ 0 & 0 & q_{34} & 1 & \cdots 0 & 0 \\ \cdots & \cdots & \cdots & \cdots & \cdots & \cdots \\ 0 & 0 & 0 & 0 & \cdots 1 & q_{n(n-1)} \\ 0 & 0 & 0 & 0 & \cdots q_{(n-1)n} & 1 \end{pmatrix}^{-1}$$

$$= \frac{\begin{pmatrix} e^{(n)}_{11} & e^{(n)}_{12} \cdots e^{(n)}_{1n} \\ e^{(n)}_{21} & e^{(n)}_{22} \cdots e^{(n)}_{2n} \\ \cdots & \cdots \\ e^{(n)}_{n1} & e^{(n)}_{n2} \cdots e^{(n)}_{nn} \end{pmatrix}}{\Delta_n} \text{..................(137)}$$

For $n = 1$, the e-matrix of Eq. 137 is 1; for $n = 2$, the e-matrix is $\begin{bmatrix} 1 & -q_{21} \\ -q_{12} & 1 \end{bmatrix}$;

and, for $n = 3$, the e-matrix is $\begin{pmatrix} 1 - p_{23} & -q_{21} & q_{21}q_{32} \\ -q_{12} & 1 & -q_{32} \\ q_{12}q_{23} & -q_{23} & 1 - p_{12} \end{pmatrix}$. In general, in the

e-matrix of degree n:

$$
\begin{aligned}
e^{(n)}{}_{uv} \text{ (for } 0 < u < \overline{n-1} \text{ and } 0 < v < \overline{n-1}) &= e^{(n-1)}{}_{uv} - p_{(n-1)n}\, e^{(n-2)}{}_{uv} \\
e^{(n)}{}_{u(n-1)} \text{ (for } 0 < u < \overline{n-1}) &= e^{(n-1)}{}_{u(n-1)} \\
e^{(n)}{}_{(n-1)v} \text{ (for } 0 < v < \overline{n-1}) &= e^{(n-1)}{}_{(n-1)v} \\
e^{(n)}{}_{un} \text{ (for } 0 < u < n) &= -\, e^{(n)}{}_{u(n-1)}\, q_{n(n-1)} \\
e^{(n)}{}_{nv} \text{ (for } 0 < v < n) &= -\, e^{(n)}{}_{(n-1)v}\, q_{(n-1)n} \\
e^{(n)}{}_{nn} &= \Delta_{(n-1)}
\end{aligned}
$$

Thus, in general, the elements of the e-matrix of any degree can be built up from the elements of the e-matrices of the lower degrees in a way analogous to the formation of the coefficients in the writer's formulas.

That Eqs. 134 yield the same results as those obtained from the matrix equations (Eqs. 135) is evident, except for the values of $a^{(n)}{}_{uv}$ and $b^{(n)}{}_{uv}$, from a study of the general properties of matrices and their adjoints as explained in the author's presentation. The values of $a^{(n)}{}_{uv}$ and $b^{(n)}{}_{uv}$ are not evident from the general matrix properties but can be derived from a special characteristic of the e-matrix, namely:

$$e^{(n)}{}_{un}\, e^{(n)}{}_{nv} = e^{(n)}{}_{uv}\, \Delta_{(n-1)} - e^{(n-1)}{}_{uv}\, \Delta_n \quad \text{.............(138)}$$

which relationship can be proved by mathematical induction.

The writer's formulas were developed some time ago independently of matrix analysis and do not depend on matrix analysis for their derivation. In deducing these formulas the writer made use of the relationship quoted by the author as Eq. 97 but in the reverse order from that used by the author—that is, the fractional form was obtained from the infinite series.

For a two-degree system, apply a unit fixed-end moment at each of the beam ends, successively, and in each case distribute in accordance with the principle of moment distribution. Fig. 9 shows such a distribution for a fixed-

1			2
d_{10}	1		
	$-d_{12}$		
		q_{12}	
		$-d_{21}\,q_{12}$	$d_{23}\,q_{12}$
	p_{12}		
$d_{10}\,p_{12}$	$-d_{12}\,p_{12}$	$q_{12}\,p_{12}$	
		$-d_{21}\,q_{12}\,p_{12}$	$d_{23}\,q_{12}\,p_{12}$
	$p^2{}_{12}$		
....	....	....	....
$\dfrac{d_{10}}{1-p_{12}} = -b^{(2)}{}_{11}$	$1 - \dfrac{d_{12}-p_{12}}{1-p_{12}} = 1 - a^{(2)}{}_{11}$	$\dfrac{q_{12}(1-d_{21})}{1-p_{12}} = -b^{(2)}{}_{21}$	$\dfrac{d_{23}\,q_{12}}{1-p_{12}} = -a^{(2)}{}_{21}$

FIG. 9.—COEFFICIENTS OF $(M'_{10} - M'_{12})$ FOR A TWO-DEGREE SYSTEM

end moment at 12. (Note that $1 - p_{12} = \Delta_2$). Other coefficients are obtained similarly. For each final end moment an infinite power series is obtained in p_{12}. The latter can be proved greater than zero and less than one, which is a necessary and sufficient condition for the convergence of the power series and the validity of relationship expressed by Eq. 97. The end moment coefficients are therefore summed up in accordance with this relationship.

The same procedure can be used for higher degree systems, but the power series becomes complicated as the degree of the system increases and the con-

vergence of the series becomes less evident. The relationships expressed by Eqs. 134 then become useful. To derive these relationships, the writer divided the n-degree system into two parts, the first part containing supports 1 to $(n-1)$, inclusive, and the second part containing support n.

Fig. 10 shows the coefficients of fixed-end moments applied at points $v(v-1)$

	1		2	
Fixed-end moment at point $v(v-1)$	$b^{(n-1)}{}_{1v}$	$a^{(n-1)}{}_{1v}$	$b^{(n-1)}{}_{2v}$	$a^{(n-1)}{}_{2v}$
Fixed-end moment at point $(n-1)n$	$-b^{(n-1)}{}_{1(n-1)}$	$-a^{(n-1)}{}_{1(n-1)}$	$-b^{(n-1)}{}_{2(n-1)}$	$-a^{(n-1)}{}_{2(n-1)}$

	$n-1$		Carry-over to second part
Fixed-end moment at point $v(v-1)$	$b^{(n-1)}{}_{(n-1)v}$	$a^{(n-1)}{}_{(n-1)v}$	$-r_{(n-1)n}\,a^{(n-1)}{}_{(n-1)v}$
Fixed-end moment at point $(n-1)n$	$-b^{(n-1)}{}_{(n-1)(n-1)}$	$-a^{(n-1)}{}_{(n-1)(n-1)}$	$r_{(n-1)n}\,a^{(n-1)}{}_{(n-1)(n-1)}$

FIG. 10.—COEFFICIENTS FOR THE FIRST PART OF THE n-DEGREE SYSTEM

and $(n-1)n$ for the end moments at the various supports of the first part of the system and for carry-overs to the second part. The carry-overs act as fixed-end moments in the second part and bring back corresponding carry-overs to the first part to act as new fixed-end moments at $(n-1)n$. The sum total of all the carry-over moments induced in the first part is an infinite power series which can be summed up by Eq. 97. The coefficients of the second part were treated similarly. The effects of both the original fixed-end moments and the summation of the infinite series of carry-overs combine to yield Eqs. 134.

The summation of the carry-overs and the final form of Eqs. 134 are conditioned on the following relationships:

$$0 < q_{n(n-1)}\,r_{(n-1)n}\,a^{(n-1)}{}_{(n-1)(n-1)} < 1 \ldots\ldots\ldots\ldots (139)$$

;

$$a^{(n)}{}_{nn} = \frac{d_{n(n+1)}\,\Delta_{(n-1)}}{\Delta_n} \ldots\ldots\ldots\ldots (140)$$

and

$$q_{n(n-1)}\,r_{(n-1)n}\,a^{(n-1)}{}_{(n-1)(n-1)} = \frac{p_{(n-1)n}\,\Delta_{(n-2)}}{\Delta_{(n-1)}} \ldots\ldots\ldots\ldots (141)$$

These relationships are interdependent and can be proved together by mathematical induction, remembering that

$$\Delta_n = \Delta_{(n-1)} - p_{(n-1)n}\,\Delta_{(n-2)} \ldots\ldots\ldots\ldots (142)$$

which follows from the peculiar characteristic of the determinant Δ.

Although the higher degree coefficients are thus obtained from those of the lower degrees, the writer has found it convenient to develop Eqs. 131 for a system of a sufficiently high degree and to apply them also to lower degree systems by equating to zero the constants of those spans which are not a part of the system under consideration.

The moments for a five-degree system (which is probably of a high enough degree for ordinary work with continuous beams) are as follows:

$$
\left.\begin{aligned}
M_{10} = {} & M'_{10} - \frac{d_{10}(1 - p_{23} - p_{34} - p_{45} + p_{23}\,p_{45})}{\Delta_5}\,(M'_{10} - M'_{12} \\
& + \frac{d_{10}\,q_{21}(1 - p_{34} - p_{45})}{\Delta_5}\,(M'_{21} - M'_{23}) - \frac{d_{10}\,q_{21}\,q_{32}(1 - p_{45})}{\Delta_5} \\
& \times (M'_{32} - M'_{34}) + \frac{d_{10}\,q_{21}\,q_{32}\,q_{43}}{\Delta_5}\,(M'_{43} - M'_{45}) \\
& \qquad - \frac{d_{10}\,q_{21}\,q_{32}\,q_{43}\,q_{54}}{\Delta_5}\,(M'_{54} - M'_{56}) \\
M_{12} = {} & M'_{12} + \frac{d_{12}(1 - p_{23} - p_{34} - p_{45} + p_{23}\,p_{45}) - p_{12}(1 - p_{34} - p_{45})}{\Delta_5} \\
& \times (M'_{10} + M'_{12}) + \frac{(1 - d_{12})\,q_{21}(1 - p_{34} - p_{45})}{\Delta_5}\,(M'_{21} - M'_{23}) \\
& - \frac{(1 - d_{12})q_{21}\,q_{32}(1 - p_{45})}{\Delta_5}\,(M'_{32} - M'_{34}) + \frac{(1 - d_{12})\,q_{21}\,q_{32}\,q_{43}}{\Delta_5} \\
& \times (M'_{43} - M'_{45}) - \frac{(1 - d_{12})\,q_{21}\,q_{32}\,q_{43}\,q_{54}}{\Delta_5}\,(M'_{54} - M'_{56}) \\
M_{21} = {} & M'_{21} + \frac{q_{12}\,p_{23}(1 - p_{45}) - q_{12}(1 - d_{21})\,(1 - p_{34} - p_{45})}{\Delta_5} \\
& \times (M'_{10} - M'_{12}) - \frac{(d_{21} - p_{12})\,(1 - p_{34} - p_{45})}{\Delta_5}\,(M'_{21} - M'_{23}) \\
& + \frac{q_{32}(d_{21} - p_{12})\,(1 - p_{45})}{\Delta_5}\,(M'_{32} - M'_{34}) - \frac{q_{32}\,q_{43}(d_{21} - p_{12})}{\Delta_5} \\
& \times (M'_{43} - M'_{45}) + \frac{q_{32}\,q_{43}\,q_{54}(d_{21} - p_{12})}{\Delta_5}\,(M'_{54} - M'_{56}) \\
M_{23} = {} & M'_{23} + \frac{q_{12}\,p_{23}(1 - p_{45}) - q_{12}\,d_{23}(1 - p_{34} - p_{45})}{\Delta_5} \\
& \times (M'_{10} - M'_{12}) - \frac{p_{23}(1 - p_{45}) - d_{23}(1 - p_{34} - p_{45})}{\Delta_5} \\
& \times (M'_{21} - M'_{23}) + \frac{q_{32}(1 - d_{23} - p_{12})\,(1 - p_{45})}{\Delta_5}\,(M'_{32} - M'_{34}) \\
& - \frac{q_{32}\,q_{43}(1 - d_{23} - p_{12})}{\Delta_5}\,(M'_{43} - M'_{45}) \\
& \qquad + \frac{q_{32}\,q_{43}\,q_{54}(1 - d_{23} - p_{12})}{\Delta_5}\,(M'_{54} - M'_{56}) \\
M_{32} = {} & M'_{32} + \frac{q_{12}\,q_{23}(1 - d_{32} - p_{34} - p_{45} + d_{32}\,p_{45})}{\Delta_5}\,(M'_{10} - M'_{12}) \\
& - \frac{q_{23}(1 - p_{34} - p_{45} - d_{32} + d_{32}\,p_{45})}{\Delta_5}\,(M'_{21} - M'_{23}) \\
& + \frac{(p_{23} - d_{32} + d_{32}\,p_{12})\,(1 - p_{45})}{\Delta_5}\,(M'_{32} - M'_{34}) \\
& - \frac{q_{43}(p_{23} - d_{32} + d_{32}\,p_{12})}{\Delta_5}\,(M'_{43} - M'_{45}) \\
& \qquad + \frac{q_{43}\,q_{54}(p_{23} - d_{32} + d_{32}\,p_{12})}{\Delta_5}\,(M'_{54} - M'_{56})
\end{aligned}\right\} \quad \ldots(143)
$$

and

$$\Delta_5 = 1 - p_{12} - p_{23} - (1 - p_{12})p_{34} - (1 - p_{12} - p_{23})p_{45} \quad \ldots\ldots(144)$$

The remainder of the moments can be obtained from the foregoing through the symmetry of the system. In matrix form,

$$\begin{Bmatrix} M_{12} \\ M_{23} \\ M_{34} \\ M_{45} \\ M_{56} \end{Bmatrix} = \begin{bmatrix} d_{12} & q_{21} & 0 & 0 & 0 \\ 0 & d_{23} & q_{32} & 0 & 0 \\ 0 & 0 & d_{34} & q_{43} & 0 \\ 0 & 0 & 0 & d_{45} & q_{54} \\ 0 & 0 & 0 & 0 & d_{56} \end{bmatrix} \frac{[e]^{(5)}}{\Delta_5} \begin{Bmatrix} (M'_{10} - M'_{12}) \\ (M'_{21} - M'_{23}) \\ (M'_{32} - M'_{34}) \\ (M'_{43} - M'_{45}) \\ (M'_{54} - M'_{56}) \end{Bmatrix} \quad \ldots(145a)$$

and

$$\begin{Bmatrix} M_{10} \\ M_{21} \\ M_{32} \\ M_{43} \\ M_{54} \end{Bmatrix} = \begin{bmatrix} -d_{10} & 0 & 0 & 0 & 0 \\ -q_{12} & -d_{21} & 0 & 0 & 0 \\ 0 & -q_{23} & -d_{32} & 0 & 0 \\ 0 & 0 & -q_{34} & -d_{43} & 0 \\ 0 & 0 & 0 & -q_{45} & -d_{54} \end{bmatrix} \frac{[e]^{(5)}}{\Delta_5} \begin{Bmatrix} (M'_{10} - M'_{12}) \\ (M'_{21} - M'_{23}) \\ (M'_{32} - M'_{34}) \\ (M'_{43} - M'_{45}) \\ (M'_{54} - M'_{56}) \end{Bmatrix} \quad \ldots(145b)$$

in which

$$[e]^{(5)} = \begin{bmatrix} 1-p_{23}-p_{34}-p_{45}(1-p_{23}) & -q_{21}(1-p_{34}-p_{45}) & q_{21}\,q_{32}(1-p_{45}) & -q_{21}\,q_{32}\,q_{43} & q_{21}\,q_{32}\,q_{43}\,q_{54} \\ -q_{12}(1-p_{34}-p_{45}) & 1-p_{34}-p_{45} & -q_{32}(1-p_{45}) & q_{32}\,q_{43} & -q_{32}\,q_{43}\,q_{54} \\ q_{12}\,q_{23}(1-p_{45}) & -q_{23}(1-p_{45}) & (1-p_{12})\,(1-p_{45}) & -q_{43}(1-p_{12}) & q_{43}\,q_{54}(1-p_{12}) \\ -q_{12}\,q_{23}\,q_{34} & q_{23}\,q_{34} & -q_{34}(1-p_{12}) & 1-p_{12}-p_{23} & -q_{54}(1-p_{12}-p_{23}) \\ q_{12}\,q_{23}\,q_{34}\,q_{45} & -q_{23}\,q_{34}\,q_{45} & q_{34}\,q_{45}(1-p_{12}) & -q_{45}(1-p_{12}-p_{23}) & 1-p_{12}-p_{23}-p_{34}(1-p_{12}) \end{bmatrix} \quad \ldots(146)$$

In conclusion, the writer is grateful for the opportunity of having the results of his own studies verified by a new approach and hopes that this discussion will contribute to the general study of the subject.

Corrections for *Transactions*: In October, 1946, *Proceedings*, on page 1098: In Eq. 42, change the x_3-values from "$-$ 5, 5, $-$ 5" to "$-$ 5, 5, 5" (that is, delete one minus sign); and, in Eqs. 43, change "$-$ 4/9" to "$+$ 2/9" in two places.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

EXPERIMENTAL OBSERVATIONS ON GROUTING SANDS AND GRAVELS

Discussion

BY JAMES B. HAYS

JAMES B. HAYS,[16] M. ASCE.[16a]—Some valuable information on the gradually increasing knowledge of grouting is contributed by this paper. The forcing of cement grout into sand has generally been considered as impossible. The author has thrown light on the subject and has indicated the approximate limiting point. The principal difficulty is to adapt the laboratory results to actual conditions as found in the field. Other considerations that must generally be taken into account are variations in gradation of the material in the layer to be grouted.

One of the difficulties encountered in using thin cement grouts is the fact that sedimentation is quite rapid in such mixes as compared with thick slurries. This is due to the settling out of the coarser particles where the slurry is not moving rapidly or is not otherwise being agitated. Therefore, a natural suggestion would be to try the use of cement screened through a fine mesh—say, the 200-mesh screen. This was done at the Owyhee (Oregon) and Boulder (Arizona-Nevada) dams for grouting contraction joints. Another suggestion along the same line would be to add a dispersion agent to the cement and note the effect in grouting. Any small increase in penetration under conditions described by the author would be highly beneficial.

The writer has always found it desirable to conduct such tests under conditions as nearly normal as possible. As described by Mr. Machis, there was no water in the column above the surface of the sand when the grout was introduced. The model might have worked differently if it had been filled with water. With a column of water 100 ft long (a common condition in practice) the coarse particles of cement would have dropped to the bottom ahead of the main stream of grout. If it is possible, the sand bed should be set vertically for the test. This is a difficult matter for the labratory.

NOTE.—This paper by Alfred Machis was published in November, 1946, *Proceedings*.

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[16a] Received December 16, 1946.

In the field, one of the most difficult problems would be to isolate the salt-water bearing zone so that the necessary grout pressure could be applied without its escape to other zones. Grout has a tendency to follow planes or zones of weak bond such as exists between steel and clay or stone and clay.

If these zones of salt water are located during the drilling of the well, there should be better means of cementing off the unwanted section than by grouting. Such methods should involve the use of certain processes well known to the driller. Deep beds of fine sand under dams could be grouted through well holes if cost were a secondary consideration; but the process would require that the grout penetrate at least several feet—rather than inches—from the hole.

If the author has the opportunity to apply his laboratory results to a field condition, the results would be interesting.

Corrections for *Transactions:* In November, 1946, *Proceedings*, Table 1(*h*), page 1207, the second line of Col. 1 should read "8" instead of 14.5; and in Fig. 3, page 1211, the word "cement" in ordinate scale (*c*) should be changed to "slurry." Turn this figure counterclockwise 90°.